THE
AMERICAN JOURNAL
OF
SCIENCE AND ARTS.

CONDUCTED BY

PROFESSORS B. SILLIMAN AND JAMES D. DANA,

IN CONNECTION WITH

PROFESSORS ASA GRAY, AND WOLCOTT GIBBS,
OF CAMBRIDGE,

AND

PROFESSORS S. W. JOHNSON, GEO. J. BRUSH, AND
H. A. NEWTON, OF NEW HAVEN.

SECOND SERIES.

VOL. XLVI.—[WHOLE NUMBER, XCVI.]

Nos. 136, 137, 138.

JULY, SEPTEMBER, NOVEMBER.

WITH FOUR PLATES.

NEW HAVEN: EDITORS.
1868.

PRINTED BY TUTTLE, MOREHOUSE & TAYLOR, 221 STATE ST.

CONTENTS OF VOLUME XLVI.

NUMBER CXXXVI.

	Page
ART. I.—Sketch of a Journey from Canton to Hankow, through China; by ALBERT S. BICKMORE,	
II.—Preliminary notice of a Scorpion, a Eurypterus? and other fossils from the Coal-measures of Illinois; by F. B. MEEK and A. H. WORTHEN,	19
III.—On the formation of Nitrite of Ammonia; by O. LOEW,	29
IV.—Notes on some Algæ from a Californian hot spring; by H. C. WOOD, Jr.,	31
VI.—On Faraday as a Discoverer; by JOHN TYNDALL,	34
VII.—Chemical Apparatus; by W. P. DEXTER,	51
VIII.—On the Equivalent of Cerium; by the late Dr. CHARLES WOLF,	53
IX.—Laws of Botanical Nomenclature adopted by the International Botanical Congress held at Paris in August, 1867; together with an Historical Introduction and Commentary; by ALPHONSE DECANDOLLE,	63
X.—On the Sulphates of Oxyd of Antimony; by W. P. DEXTER,	78
XI.—On the Secular Variation of the Elements of the Earth's Orbit; by JOHN L. STOCKWELL. (With a plate),	87
XII.—On some Cretaceous fossil Plants from Nebraska; by L. LESQUEREUX,	91
XIII.—Recent Eruption of Mauna Loa and Kilauea, Hawaii,	105

SCIENTIFIC INTELLIGENCE.

Chemistry and Physics.—On a new mode of forming the organic sulphacids, STRECKER: On the transformation of uric acid into glycocoll, STRECKER, 124.—On an oxychlorid of silicon: On a universal method of reducing and saturating organic bodies with hydrogen, BERTHELOT, 125.—Carbonylic Sulphid, BERTHELOT, 129.—Test for naphthalin, VOHL: A Manual of Inorganic Chemistry arranged to facilitate the Experimental Demonstration of the Facts and Princi-

ples of the Science, by CHARLES W. ELIOT and FRANK H. STORER, 130.—On a mode of extracting the metals Molybdenum and Chromium; by J. ENEU LOUGHLIN, 131.

Mineralogy and Geology.—A system of Mineralogy; Descriptive Mineralogy, comprising the most recent discoveries, by J. D. DANA aided by G. J. BRUSH, 132.—The Mining and Metallurgy of Gold and Silver, by J. ARTHUR PHILLIPS, 134.—New Geological Maps and Chart: New borate from Mine Hill, Franklin, Sussex Co., New Jersey—Sussexite; by G. J. BRUSH, 140.

Botany and Zoology.—The Variation of Animals and Plants under Domestication, by CHARLES DARWIN, 140.—A second critical notice of Alcyonaria in the Museum of Yale College; by A. E. VERRILL, 143.—Note on Ethmophyllum and Archeocyathus; by F. B. MEEK, 144.

Astronomy.—Theoretical Astronomy relating to the motions of the Heavenly Bodies revolving around the Sun in accordance with the law of Universal Gravitation, by JAMES C. WATSON, 145.—Astronomical and Meteorological Observations made at the United States Naval Observatory during the year 1865, 146—Small Planets, 147.

Miscellaneous Scientific Intelligence.—Petroleum Wells in Mexico, 147.—American Association for the Advancement of Science: British Association, 148.—*Obituary.*—Anton Rosing, 148.—Julius Plücker: Charles G. Page, 149.

Miscellaneous Bibliography.—A treatise on Meteorology; with a collection of Meteorological tables, by ELIAS LOOMIS, 149.—The Butterflies of North America, by WM. H. EDWARDS: Observations on Polyzoa, Suborder Phylactolæmata, by ALPHEUS HYATT, 150.—The Workshop: Transactions of the American Entomological Society, Philadelphia, 151.

Proceedings of Societies, etc., 151.

NUMBER CXXXVII.

	Page.
ART. XIV.—A new Theory of Vision; by SAMUEL ROWLEY,	153
XV.—Fundamental Principles of Molecular Physics; by Prof. W. A. NORTON,	167
XVI.—On Faraday as a Discoverer; by JOHN TYNDALL,	180
XVII.—On Enargite from the Morning Star Mine, California; by EDWARD W. ROOT,	201
XVIII.—Physical Observations on the Andes and the Amazons; by JAMES ORTON,	203
XIX.—Notes on the Caucasus; by Capt. F. VON KOSCHKULL,	214
XX.—On some points in the Geology of Vermont; by T. STERRY HUNT,	222

	Page.
XXI.—Contributions from the Sheffield Laboratory of Yale College. No. XVII.—On Willemite and Tephroite; by WILLIAM G. MIXTER,	230
XXII.—Notices of papers in Physiological Chemistry. No. I. By GEORGE F. BARKER,	233
XXIII.—Contributions from the Sheffield Laboratory of Yale College. No. XVIII.—On Sussexite, a new borate from Mine Hill, Franklin Furnace, Sussex Co., New Jersey; by GEORGE J. BRUSH,	240
XXIV.—On an easy and very effective mode of showing the vibrations in Chladni plates, &c., to a large class, by the use of a calcium or electric lantern; by JESSE S. CHEYNEY,	243
XXV.—On new specimens of Eozoon Canadense, with a reply to the objections of Professors King and Rowney; by J. W. DAWSON, and notes by W. B. CARPENTER. With two plates,	245
XXVI.—On Aquacreptite, a new mineral, and on Corundophilite of Chester; by CHARLES UPHAM SHEPARD,	256
XXVII.—A new locality of Meteoric Iron in Georgia; by CHARLES UPHAM SHEPARD,	257

SCIENTIFIC INTELLIGENCE.

Chemistry and Physics.—Hinrichs's Atom Mechanics, FLECK, 258.—On Vortex-rings in Air, BALL, 260.

Mineralogy and Geology.—Twentieth Annual Report of the Regents of the University of the State of New York, etc., 262.—On a new large Enaliosaur, COPE, 263.—Bone caves of Brazil and their animal remains, REINHARDT, 264.—Paleozoic Entomostraca, JONES and HOLL: On the discovery of a new Pulmonate Mollusk in the Coal-formation of Nova Scotia, with a description of the Species, DAWSON and CARPENTER, 265.—Notes on Hetch-Hetchy Valley, HOFFMANN, 266.—Acadian Geology; The Geological structure, Organic remains and Mineral Resources of Nova Scotia, New Brunswick, and Prince Edward Island, DAWSON, 267.—On Subaërial Denudation, and on Cliffs and Escarpments of the Chalk and the Lower Tertiary Beds, WHITAKER, 268.—Death of Fishes on the coast of the Bay of Fundy, ADAMS: Geological chart of Southern Norway, representing the dioceses of Christiania, Hamar, and Christiansand, KJERULF and DAHLL, 269.—Revue de Géologie pour les années 1865 et 1866, 270.

Botany and Zoology.—The Book of Evergreens, a practical Treatise on the Coniferæ or Cone-bearing Plants, HOOPES, 270.—The Miscellaneous Botanical Works of Robert Brown, BENNETT: The Journal of the Linnean Society, Botany, BENTHAM, 271.—Théorie de la Feuille, DECANDOLLE, 272.—Obituary announcement, WARD and ARNOTT: Note on the Polymorphism of the Anthozoa and the Structure of

the Tubiporæ, KÖLLIKER, 273.—A Guide to the study of Insects, and a Treatise on those injurious and beneficial to Crops, PACKARD, 274.

Astronomy.—Notice of new Planets (98), (99) and (101), WATSON, 274.—Discovery of another new Planet by Dr. Peters: Elementary Lessons in Astronomy, LOCKYER: Tables for the mutual conversion of Solar and Sidereal time, LANG, 275.

Miscellaneous Scientific Intelligence.—Seventeenth Meeting of the American Association for the Advancement of Science, 275.—On the fall of Rain, as affected by the Moon, CHASE, 281.—On a supposed connection between the amount of Rainfall and the changes of the Moon, HENNESSEY, 283.—On a new Meteorite, GEINITZ, 284.

Obituary.—Professor Matteucci, 285.

Miscellaneous Bibliography.—Cambridge Physics. A Hand-book of Natural Philosophy, ROLFE and GILLET, 285.—Braithwaite's Retrospect of Practical Medicine and Surgery: Ranking's Half-Yearly Abstract of the Medical Sciences: Archives du Musée Teyler: The American Naturalist, 286.—Reliquiæ Aquitanicæ, LARTET and CHRISTY: A treatise on concentration of all kinds of ores, including the chlorination process for gold-bearing sulphurets, arseniurets, and gold and silver ores generally, KUSTEL: Catalogue de Minéraux, Roches, Fossiles, Modèles en Platre et Modèles en Bois des cristaux à vendre dans le Comptoir Minéralogique Rhénan, KRANTZ, 287.

Proceedings of Societies, etc., 287.

NUMBER CXXXVIII.

	Page.
ART. XXVIII.—On the molecular structure of Uric Acid and its derivatives; by WOLCOTT GIBBS,	289
XXIX.—The Occultator; by THOMAS HILL,	299
XXX.—On the Amiens Gravel; by ALFRED TYLOR,	302
XXXI.—On the Artificial Formation of Organic Substances; by C. GREVILLE WILLIAMS,	327
XXXII.—Notes on the Caucasus; by Capt. F. VON KOSCHKULL,	335
XXXIII.—Notes on Mr. Charles Stodder's paper entitled "Nobert's Test-plate and modern Microscopes;" by W. S. SULLIVANT,	347
XXXIV.—Remarks on the nineteen-band Test-plate of Nobert; by J. J. WOODWARD,	352
XXXV.—Notes on the Geology of Southwestern Ontario; by T. STERRY HUNT,	355
XXXVI.—On the action of Sunlight on Bisulphid of Carbon; by O. LOEW,	363

Page.
XXXVII.—Observations on the Metamorphosis of Siredon into Amblystoma; by O. C. Marsh, ... 364
XXXVIII.—Notice of a new and diminutive species of fossil Horse (Equus parvulus), from the Tertiary of Nebraska; by O. C. Marsh, ... 374
XXXIX.—On Hansen's Theory of the physical Constitution of the Moon; by Simon Newcomb, ... 376
XL.—Notices of papers in Physiological Chemistry—No. II; by George F. Barker, ... 379
XLI.—Observations of the Auroras of Sept. 5th and 15th, 1868, at Mt. Desert Island, Maine; by W. S. Gilman, Jr., 390
XLII.—Discoveries of new Planets; by J. C. Watson, ... 392

SCIENTIFIC INTELLIGENCE.

Chemistry and Physics.—On the combustion of hydrogen and carbonic oxyd in oxygen under great pressure, Frankland, 394.—On the reduction and saturation of organic compounds by hydrogen, Berthelot, 395.—On the persulphid of hydrogen, Hofmann, 396.—On the compounds of molybdic and phosphoric acids, Debray, 397.—On iodid of silicon and silico-iodoform, Friedel: On the density of the vapor of calomel, Mitscherlich, Deville and Troost: Connotations of Magnetism, Chase, 398.

Mineralogy and Geology.—On a new Mineral in Cryolite, Rand, 400.—Notes on the Later Extinct Floras of North America, with Descriptions of some New Species of Fossil Plants from the Cretaceous and Tertiary Strata, Newberry, 401.—On Human remains along with those of the Mastodon in the drift of California, Winslow, 407.—Meek on the shell structure of some Spirifera, 408.

Botany and Zoology.—Botanical Notices, 408.—Contributions to the Fauna of the Gulf Stream at great depths, Pourtales, 409.—Deep Sea dredgings in the region of the Gulf Stream, Pourtales, 413.—On the genus Lælaps, Cope, 415.—Considerations drawn from the study of Mole Crickets, Scudder, 417.—Remarks on two new fossil Insects from the Carboniferous Formation in America, Scudder, 419.—Siredon a stage in the development of Amblystoma, Silliman, 421.

Miscellaneous Scientific Intelligence.—Earthquake of Aug. 13th—16th, 1868, 422.—Earthquake in California, Oct. 21: List of Papers read at the session of the National Academy of Sciences in August, 1868, at Northampton, 428.—On a Meteorite which exploded over Kansas, June 6, 1868, Mudge, 429.—Corrigenda for Article XIV on Vision: British Association, 430.

Miscellaneous Bibliography.—Report of J. Ross Browne on the Mineral Resources of the States and Territories west of the Rocky Mountains, 430.—Report of James W. Taylor on the "Mineral Resources of the United States east of the Rocky Mountains," 431.—A treatise on the Concentration of all kinds of Ores, including the Chlorination Process for Gold-bearing sulphurets, Arseniurets and Gold and Silver ores generally, Kustel: The American Annual Cyclopedia and Register of Important events of the year 1868: The Wine Maker's Manual, Reemelin, 432.—

Chambers's Encyclopedia: a Dictionary of Universal Knowledge for the people, 433.—Mitchell's Manual of Practical Assaying, CROOKES: The Workshop: a monthly Journal devoted to the progress of the useful arts, STEIGER, 434.—Thesaurus Siluricus: the Flora and Fauna of the Silurian Period, with Addenda, BIGSBY: Exposé des Formations Quaternaires de la Suède, ERDMANN: Prof. Safford's report on the Geology of Tennessee: The American Entomologist, WALSH and RILEY, 435.—How Crops Grow. A Treatise on the Chemical Composition, Structure and Life of the Plant, for all Students of Agriculture, with numerous illustrations and tables of analyses, JOHNSON: A Treatise on Optics; or Light and Sight theoretically and practically treated; with the application to fine art and industrial pursuits, NUGENT: Part 2 of the Butterflies of N. America, EDWARDS: Note, 436.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. I.—*Sketch of a Journey from Canton to Hankow, through China;* by Albert S. Bickmore, A.M.

[Read before the Royal Geographical Society of London, Dec. 8, 1867, and before the Boston Society of Natural History, Feb. 19, 1868.]

On the 7th of August, 1866, I left Canton, in company with Mr. C. L. Weed, photographer at Hongkong, and Rev. Mr. Nevin of Canton, on a journey through the province of Kwangtung and the eastern part of Kwangsi. Our course, at first, was westward, for about sixty miles, when we reached the head of the great Delta of the Sikiang, whose low, fertile fields spread out widely along the river banks, and support a most dense population. Along the borders of these low lands, rise serrated mountains—some peaks attaining an elevation of fifteen hundred to two thousand feet—their sharp ridges and projecting spurs coming out in strong relief, on account of the scanty vegetation on their sides. To one who has been journeying in tropical lands, and especially among the luxuriant forests of Sumatra, these mountains appear surprisingly bare, and only the more so, when he considers that he is but on the verge of the temperate zone.

This nakedness appears to be a universal characteristic of the mountain scenery in China, but it is not the fault of the soil or the climate, for wherever the little pines have been suffered to rise, they show a vigorous growth. The cause of this universal devastation is the frequent rebellions that have swept back and forth over the whole empire, like a desolating flood.

In a few days the Chinese can rebuild their low mud houses; but Nature requires years to cover her mountains with forests; and rebellion has followed rebellion too quickly for her to accomplish the ever recurring task; and besides, the people do not care to labor much, when there is every probability that outlaws or robbers from a neighboring province will profit by their industry. Yet it is true they do raise some trees in a few places; but over all the wide area that I have traveled, not a tenth part of the soil is thus improved that might be; and then the trees are generally cut down before they attain any considerable size; and this, in districts where the population is numbered by the hundred thousand. The grand old trees which are occasionally seen around the Buddhist temples, owe their preservation only to the superstitions of the destroyers, and these show well what splendid timber thousands of hill-sides in China might yield.

But in regard to the low lands, it scarcely seems possible that they could be made to produce more than is already raised—two full crops being obtained in nearly every part of the empire. The continued fertility of these lands has long been a wonder to the world. It is due no doubt chiefly to two causes; first, the Chinese are careful to save everything that can possibly serve for manure—in some places even to the hair they shave from their heads; and secondly, their low lands (where all their rice and most other sustenance is obtained) are all, or very nearly all, subject to floods at least once a year, and a rich deposit of fine mud is thus spread over them, just as in the valley of the Nile.

Following up the Sikiang, through a deep pass in the first mountain range, we came to the city of Shauking, where the Viceroy of the provinces of Kwangtung and Kwangsi resided when the Portuguese first appeared off the coast. About two miles behind it rise the famous "Marble Rocks," or "Seven Stars," like dark, sharp needles, out of the low, green plain. Mr. Nevin and I measured them with an aneroid barometer, and found them to range from one hundred and fifty to three hundred feet high, though they had been previously estimated at twice that height. The rock of which they are composed is a highly crystalline limestone, of a dark blue color on the weathered surfaces, and a rusty iron tinge where large fragments have been lately detached, the whole traversed in every direction with milk-white veins, and completely fissured through and through, in every direction, by joints and seams. Over the whole exterior they are extremely rough and jagged, and furrowed by perpendicular grooves, worn by the small streams that course down their sides during every slight shower.

They form such striking objects in the surrounding plain as the "Little Orphan" does in the waters of the Yangtse, and, like it, abound in groups of little temples placed in the natural niches in their sides. Larger temples are ranged at their feet, and one which we entered contained in the principal hall three images in bronze six or seven feet in height. In another room I noticed an idol with six arms. The whole building was going rapidly to decay, and it was only after much searching that we succeeded in finding two poor old monks, preparing a scanty meal in the refectory,—the last place they were willing to desert in the whole temple.

Climbing up a steep, narrow stair-way, that rises diagonally across the face of the precipice, we reached a second temple perched high in a little rock. Along a part of this stair-way, a rude heavy chain was fastened to the mountain side, that the timid and weary might help themselves on to the temples above; and many must have been the pilgrims that ascended this difficult way, if we could judge from the depth of the places their feet had worn in the steps of the solid rock. The entrance to the temple was through a crazy gate-way or portal of loosened bricks, that leans over the precipice, and threatens to fall with the first person who sets foot within it and immolate him to a heathen god. This temple we were informed was built some two hundred years ago when Shauking was a great and flourishing city, but now the monks can scarcely beg enough from their poor neighbors to answer their immediate necessities, and their once splendid temples are rapidly becoming only unsightly heaps of ruins.

Here, as is frequently the case in masses of limestone, are several caves. We entered one of a bell shape. Its floor was mostly covered with water, and a bridge led us to a platform at the farther end. As we were crossing this Stygian stream, we were saluted with a fierce barking, and certainly we did seem to be approaching the regions over which Cerberus presides, but no other charm was needed for us to safely pass these canine guardians than a threatening show of our canes. Many tablets have been cut in the rocky sides and along a stair-way that led up to where the cave opens out to the sky at the opposite side.

During these excursions in the vicinity of Shauking, both of my companions became quite ill, and at my earnest solicitation finally consented to return and let me continue on alone.

On the second day from Shauking, I came to "Cock's Comb Rock," a huge wall or dike, of black or crystalline marble, with a crest so jagged that the name the Chinese have given it exactly describes it. Northwest of this, in a small plain, is

a conical hill of limestone, whose whole interior has been washed away, forming a much grander cave than the one we had previously visited in one of the "Seven Stars."

All the mountains in these regions are composed of fine, hard siliceous grits, which in some places are compact and flinty, becoming true quartzite or quartz rock, and in others are soft as sandstone; and besides these, of slates that are interstratified with these grits and are sometimes soft clay slates, and at others as hard as shales. Half a mile below the village of Kok-hau, on the left bank of the Sikiang, just before I reached the boundary of the province of Kwangsi, I found these grits and slates resting *immediately on granite.* Two miles below Kok-hau rises "Ornamental Monumental" Rock. It belongs to the lower part of this series of grits and slates, but is composed of a coarse conglomerate, and perhaps represents the conglomerates that are found near granite, in other parts of the empire.

Crossing the river from Cock's Comb Rock, we came to a small village, and anchored for the night astern of a small gunboat. On consulting my chart I found these words written around the next bend, about half a mile up the stream,—"*a favorite resort for robbers!*" But I believed we must be safe with a gunboat so near, and taking care that my revolver was in prime order, and that a heavy sword was within my grasp—a provision that was constantly repeated every night of my long journey—I laid down determined to sleep, despite a continual din of tam-tams, and the most extravagant shrieking and groaning of some women on the bank, who were lamenting the decease of a friend.

Late in the night the watch began calling out loudly, then my servant and my boatmen joined in. A strange boat was stealing along toward us, and although it was already so near that I could hear the noise of its approaching oars there was no reply. At the next instant the gunner on the watch-boat fired his cannon and at once the men in the suspicious boat all answered in the meekest and most humble tones. Our would-be robbers had that time mistaken their prey. This is but a fair example of the noises and alarms that were constantly recurring by night all the way to Hankow.

As we slowly ascended the river by poling, tracking and sailing, we stopped several times a day, that I might collect specimens of the rocks and ascertain the dip and strike of the strata. In this way, in a week we came to Wuchau, the last missionary outpost in this direction. Here I met the Rev. Mr. Graves, and induced him to accompany me up the Kweikong, or Cassia river, as far as Kweilin, the capital of the province of Kwangsi.

It is so dangerous ascending this river, on account of robbers, that boats leave Wuchau only when several are ready to go and can keep together and afford each other mutual assistance in case of an attack. As an additional protection, the Mandarin offered to send a small gunboat along with us, but when we were ready to go, only one policeman appeared and he carried no arms.

The boats used on this river are quite different from those seen at Canton. They have flat bottoms and curve up high at the bow and stern, that the helmsman and a man on lookout forward may see some distance ahead and avoid the rocks, as they come down with the rapid current.

The principal article carried up the river by these boats is salt, which is a government monopoly; and, notwithstanding our boatmen all agreed not to bring a particle on board, they did buy a considerable quantity, and tried to hide most of it in our part of the boat. We very plainly informed them they had not kept their agreement, and if they left it there it would instantly go overboard. They finally, as near as I could ascertain, bought a permit for a part of it and smuggled through the rest. This smuggling is so common, that I was repeatedly informed that the Mandarin boats, which are not liable to be searched by the custom-house officers because they carry high officials, never fail to improve every opportunity to avoid paying the regular tax.

As we passed along these rivers, every day or two we came to a small house with two poles in front, each bearing a large triangular flag. There we were obliged to stop, and allow our boat to be searched by fierce looking fellows, each armed with a long stick pointed with iron. Ascending this Cassia river is little better than dragging a boat up one continued series of rapids; and though ours drew but five or six inches, it seemed sometimes as if our boatmen would be quite unable to get her along any farther. This fact indicates the shallowness of the stream, and also the unfavorable fact, that steamers can never be used on this river. The boatmen at Wuchau calculate to reach Kweilin in fourteen days, but to go all the way back in four.

For the first hundred miles we passed only small, scattered villages, each having on the top of the highest hill near it a fort, where they keep all their extra rice and clothing—every thing they use from day to day—for every village pillages on every other village, and on all the boats that pass, whenever they dare. These fortified hill-tops reminded us of the pictures drawn by historians, of the middle ages; but these people observe even less law and order than those of those early times.

As an illustration of the complete state of anarchy that obtains throughout all this region, I may mention that on our third day from Wuchau we passed a large *Mandarin* boat that had been robbed of everything the very first night after leaving Kweilin, the officials even not being able to protect themselves from these desperate thieves.

All along our route the Mandarins were very kind to us, but kept asking how we could dare to come there, where only one foreigner had ever been before, and who, though he had escaped the people of Kwangsi, was murdered by the people of Hunan. They referred to an eccentric genius, who did succeed in reaching Hankow, but was completely stripped of all his clothing. His difficulty with this people was certainly one cause of their hostility to us.

Beyond the hien city of Chauping, the country becomes somewhat more cultivated, yet even here it is very sparsely peopled, and there is no need that a single man should leave China to find plenty of good land to cultivate.. The river here flows through deep passes, and we entered one called "Forest Pass," as the bright day was darkening into twilight. The rock was a hard siliceous grit, and sharp peaks in the range rose up to a height of 1600 or 1700 feet. Like the famous Shauking Pass, this is also a cleft in a mountain range; but while that is about 600 yards wide, this is only from 50 to 150, and as we sailed between the high, almost overhanging precipices, the effect was far grander than anything I have ever enjoyed in China.

As night overtook us while in the pass, we moored our boat to some huge rocks by the steep bank, and then climbed to the edge of a neighboring ridge and waited for the full moon, whose soft light was just then brightening the eastern sky; and when her silver disk rose over the jagged edge of the high peaks above us and threw long pointed shadows down the steep-sided pass, we had before us such a view as a lover of crayon sketches might well roam the whole world over to enjoy.

As we approached Pingloh, a high range of needle-shaped peaks stretched across the river from east to west. They were composed of the same dark blue, highly crystalline limestone traversed with white veins, that had been previously noticed in the Seven Stars at Shauking and Cock's Comb Rock on the Sikiang. Here the pass gave a section showing this limestone resting on the previously mentioned grits. In the muddy places were large quantities of a beautiful blue *Convolvulus* in full bloom, of the same species as specimens Mr. Graves had frequently found in the limestone caves near Shauking.

Our daily routine was to walk in the forenoon until the sun got high, and again in the afternoon until the boat reached a safe anchorage, Mr. Graves collecting plants and sketching a map of the river, and I gathering geological specimens, ascertaining the dip of the strata and the direction of the elevations, details too numerous to be given in full in this hasty sketch.

On the evening after leaving Pingloh we were following the river as it bent around a high bluff, when we suddenly found ourselves on the edge of a valley, ten or twelve miles broad, and extending farther than we could see to the right and left. In every direction this whole valley was perfectly *bristling* with sharp peaks of limestone. The strata of this limestone were nearly horizontal and once the whole valley was filled with solid rock, which, in the course of ages, had been worn into deep channels that have been kept widening until only sharp peaks are left of what were originally continuous sheets of rock. From a single position on the low river bank I counted *one hundred and ninety-two* separate peaks. The highest I judge rose 1200 feet above the plain,* but even this did not represent the original depth of the deposit. These dark rocks, rising abruptly up from the low, level lands at their feet, contrasted most strikingly with the bright light green of the fertile plain and made this view the most picturesque and remarkable seen on this journey. A similar view is to be enjoyed among the contorted and fractured Devonian rocks on the banks of the Tchussovaya, on the western flanks of the Ural; and it is probably to this same Devonian period that these limestones, and those previously mentioned, belong.

On passing out of this limestone region, a section was obtained a little above the market-place, Hingping, where these limestones were seen resting (*conformably* as near as I could ascertain) on the grits that at Kokhau were in them found resting on granite.

About Kweilin, the capital of the province, the valleys are much broader and better cultivated; and large water-wheels, twenty or thirty feet in diameter, are common along the river banks where the rapids are strong enough to keep them moving. Pieces of bamboo are fastened to the rim at a slight angle, and bring up the water and pour it into a trough as they reach the highest point and begin to descend on the revolving wheel.

A small pagoda, perched on the top of a ragged rock, and a

* As we could not learn that this peak was known by any particular name, I propose to name it Longfellow's Peak. This and all the surrounding limestone needles appear like high columns that once supported the roof of one immense temple.

high wall of limestone, through which had been chiseled a large hole, were pointed out by our boatmen as indications that we were nearing the capital of the province of Kwangsi. Instead of being situated on the west side of a lake, as represented on the best maps, we found it on the west side of the Kweikong river, which in the rainy season probably overflows its banks. The walls of the city are of limestone blocks neatly cut, with a parapet of bricks.

We carefully closed our boat and in the evening rowed up to the city. I at once dispatched my servant to the Yamun—as the Chinese call the place where their officials reside—to ask for chairs and policemen to protect me as far as the next city, but all arrangements could not be completed till the next day. Meantime we were careful not to let any one see us, but in some way they found we had come, and early the next morning all the streets and boats near us were perfectly packed with people anxious to get a sight at the foreigners. At first we tried to escape by ordering our boatmen to move, first to one place and then to another, and thus we darted hither and thither like a bird trying to escape from a hawk, but everywhere we found a greater and greater throng, and finally we concluded it was best to try to partially gratify their insatiable curiosity by going out on to the forepart of the boat and exhibiting ourselves by turns.

When one crowd had satisfied their desire to see "the barbarians," they generally left us but they were immediately replaced by one still larger, until it seemed as if all the Chinamen south of the Great Wall had come out to gaze at us. Meanwhile my servant arrived from the Yamun, saying that all was ready, and the Mandarin begged if I was going to Hankow that I would depart at once, for the whole city was so much excited by a proclamation issued by the gentry, that he feared we would be attacked and he would not be able to defend us.

Mr. Graves kindly translated the proclamation for me. It ran as follows :

> "It has already been determined, by common consent, that if any one has anything to do with the Imps, or rents them a house or any other dwelling place, his house and his family dwelling shall be immediately burnt to the ground, and his whole family, male and female, old and young, shall be at once put to death.
>
> "By Order of the whole Provincial City."

Despite this formidable threat, I determined to continue to push my way through to Hankow, or perish in the attempt.

A great crowd gathered on the shore where I landed and the boys hooted and shouted, but I could not understand what they said and only hurried on my chair bearers through the suburbs, which were everywhere perfectly thronged. Two or three times

I feared they would block up the street in front of me and stop me completely, but they seemed to have a suspicious regard for the barbarian and concluded to allow me to pass on.

When we came to the chief gate and were entering the city, some officials stopped my chair and drew me up into their office out of the press of the crowd, while they were instructing my coolies to go *round* the city and not *through* it. One of my chair bearers took this opportunity to run away and it seemed an age before another could be found; but finally I continued on between the city wall and the river until we came to a great rock, round which we were carried in a boat, and thus we were at last freed from our tormentors. It gave me a most delightful sensation to find myself once more able to walk, so that I might hurry on my chair bearers to the top of their speed and thus, with all possible haste, distance this City of Destruction! Night, however, overtook us when we were five miles away and the two policemen guarding us selected an inn in a little village, where we lodged for the night.

After such a frightful tumult, it seemed so safe and so sweetly quiet that I was tempted out a little way into an adjoining field to note, by aid of my azimuth compass, the direction of the valley we were to travel in on the morrow and the form of the mountains that bordered it. While I was thus absorbed in the beautiful view before me a villager chanced to pass by and notice my open compass, so I shut it up and went back to rest for fear he might think I was like some of their own people—a geomancer. Later in the evening the whole neighborhood began to resound with a heavy beating of gongs, and soon a large crowd gathered in front of the inn, shouting out in the most fiendish tones, "*Kill him! Kill him! Kill the white devil!!*" I plainly saw that they had come with the determination to rob me and then kill me. I realized the danger of my position and I feared the worst, for how could one man defend himself against such an infuriated mob.

But my policemen proved firm, and at once showed the ringleaders my pass from their own Mandarin and assured them that if they injured me in the least their Mandarin would take every one of their heads off and completely destroy their village. Then their wrath took another channel and they cursed the Mandarin, and finally, after much angry disputing, they offered to go away on the condition that I should leave their village as soon as daylight appeared. My servant assured them that they need have no fear that I should remain *there* long, and that I certainly should not have stopped short of the next village if my coolies and policemen had not refused to travel any farther that night. The only crime alleged against

me was, that one of their number had seen me with a mysterious instrument observing the mountains and valleys, and that they were all satisfied that I had come to take away the treasures which they believed their land possessed. Almost everywhere over the whole empire, wherever they saw me breaking the rocks or observing the dip of the strata, they at once concluded I was searching for gold or silver or precious stones. Another common belief is that a foreigner who has blue eyes certainly has the clairvoyant power of looking three feet into solid rock!

The next day at early dawn we started up the valley to the north-east, the general direction of the Kweikong above Kweilin. The road, or more properly path, was three or four feet wide and paved with blocks of limestone and small rounded boulders from the bed of the neighboring river. Large numbers of coolies were passing to and fro, this being one of the four great highways between the southern part of the empire and the valley of the Yangtse. The others are that from the province of Kwangtung over the Great Meiling Pass into the province of Kiangsi and down the Kan river to Poyang Lake; another from Shauchau to Lohchiung and over the Lesser Meiling Pass; and the fourth from Yunnan, the capital of the province of that name, to Kweiyang the capital of Kweichau and thence down the Wu river to the Yangtse.

At 10 A. M. the road came to a small tributary of the Kweikong coming in from the northwest. On each side of this stream there had once been a large flight of marble steps nicely cut and carefully laid up, but when I passed they were all falling apart and the whole work going to decay, the amount of travel at present not being sufficient even to keep them in repair.

On one bank was a small square pagoda-like tower, and near it two great iron pillars surmounted by a large ornamental cap. Around each pillar was an iron ring and to this was attached a series of huge chains large enough to anchor a sea junk. The people said they were to fasten robbers to, probably the Miautse who live in the neighboring mountains to the north and northwest and who are said to come down frequently and plunder the smaller villages. Notwithstanding this formidable array of iron pillars and great chains, these Miautse have maintained an uninterrupted independence, a proof of the continued weakness of the Chinese government in every dynasty.

At 2 P. M. we stopped to lunch at a small inn. The policemen insisted on my going into a small room and remaining there out of sight till we were ready to start again, and after that all the way to Hankow, a distance of some 800 miles, I was so

strictly guarded and attended that I found myself really a prisoner. I could not make detours to the right and left as I pleased when we were passing some object of special interest. My compass I was compelled to secrete under my waistcoat, and I dared to use it only when we were far away from any village, and the road clear of coolies, and even then my policemen generally manifested a belief that I was little better than a spy, and my servant always felt it his duty to remind me how nearly my using it before had come to costing both of us our lives.

2 P. M. we came to Lingsun a hien city, 60 li by the way we came from Kweilin. I must confess that a sickening sensation closely akin to fear crept over me as I entered the gate of this city and thought of the danger I had passed through the day before at Kweilin.

The Yamun was near the gate we entered, and the officials that quickly gathered round all seemed to regard me with pity rather than hate. I tried to show my appreciation of the kind feelings they manifested by naming the places I had passed and marking out a rude map on the wall, but my policemen were afraid another mob might gather and therefore led me away to a little dirty inn where every room was full but one, and on one of the two beds in that an old opium smoker lay stretched out nearly stupefied with the intoxicating drug. Our room was more properly a dungeon than a guest chamber. A single fragment of glass in the roof, which was little higher than our heads, admitted all the light we were permitted to enjoy. But my companion, at least, was blissfully indifferent to the inconveniences of our prison and no doubt was imagining himself floating on clouds in the high air or in some richly gilded barge quietly gliding down Lethe's stream, whose waters he had certainly drank to satiety.

Small boys climbed up the partitions to peep over and steal a sight at me, but I was then quite accustomed to such slight annoyances. Meanwhile numbers of the curious of both sexes gathered in an outer room, and as a cloud of dust rose from the dry matting on the *kang* whenever I moved, it soon irritated my nostrils, and the whole party outside set up a loud laugh to think that such a strange animal could *sneeze!*

After three hours in these uncomfortable quarters, we continued on through the city and passed out the eastern gate. The whole city is merely one heap of ruins and there are scarcely houses enough left to line the main street, so complete is the destruction made by the Taiping rebels. In fact all the way from Chauping I had come almost exactly in the track of these rebels, and their hordes were composed of just such rob-

bers and desperadoes as I had found there. Their leader was undoubtedly stimulated to his undertaking by chagrin at not being able to pass the government examinations, almost the only way open to the subjects of that empire to obtain honor and wealth. It would be strange indeed if, as some people suppose, a man whose prime motive was to take revenge on his government should care much about elevating his countrymen. It is true he and his confederates invited foreigners to participate with them in overthrowing the dynasty of the Manchus, but I believe that they did this only because they needed assistance, and that if they had once gained the supreme authority they would have been as hostile to foreigners as the present dynasty; and a partial proof of this appears in the reserved manner in which their chief conducted himself as soon as he had secured Nanking and believed the whole empire within his grasp. This territory where the "Great Peace" rebellion began, and the territory too that they held the longest, is the most despoiled, the most dangerous and the most unpromising of any I have seen in my long journeys over China. Revolution has followed revolution throughout the whole length and breadth of China until her soil has been reddened with the blood of tens, even hundreds of millions of her people, and yet she remains just where she was two thousand years ago; and simply because all these movements have been originated by those whose only desire was to get the throne, to plunder or to avenge personal wrongs; and not by high-minded, generous men, having in view the good of their fellow countrymen.

A walk of 35 li brought us to Tai-ung-gong, a small village on the Kweilin or Cassia river, for the water still flows toward Kweilin. Before we reached it we crossed a small stream flowing into the Kweikong from the north. In its bed I noticed pebbles of granite and porphyry, but all the rocks seen *in situ* were the common siliceous grits. Here were many rafts of bamboos to be floated down to Kweilin and Wuchau. The valley here is filled with small hills, but in this place only. Among them I gathered a beautiful blue bell quite like that found on our own hillsides in New England. A kind of blackberry that grew in the old ruins by the way side was just ripening, and the opening of the asters also heralded the coming of autumn, as at home. I had chosen the best season of the year for this journey, for there is far less danger from the people when it is harvest time and they are not suffering from want of food. The next day we traveled 55 li to Hingan, a hien city. The water here flows to the north and the water-shed is a few li to the southwest. It is not natural but artificial, and what were originally small streams have been changed into canals and

these extended to head waters of the Siang. The water is kept for a time in these rivers and canals by building dams across them whenever a rapid would occur and allowing the water to escape only over a small gap deep enough for a single boat to pass over. Hingan is in the same ruinous condition as Lingsun. It is 150 li in a northeasterly direction from Kweilin. The Siang here was so low that I had to go 17 li farther to the village of Tankatse to take a boat for Sinchau, 140 li down the river. All along this route the water was so low that we were continually thumping and bumping and grating over the rocks and coarse shingle, especially in the dams which we met every one or two li. They are made with a gap for the boat to pass over near one bank and near the other a sluice-way, where as many as ten great water wheels were sometimes seen, one behind the other. It seemed as if there were more rapids in the 14 leagues from Tankatse to Sinchau than in the 16 leagues from Kweilin to Wuchau on the other side of the water-shed. Sinchau is the chief city in this region and appeared nearly as large as Wuchau. It is not in the province of Hunan but in Kwangsi. At the boundary of these provinces dark shales appear apparently resting on the limestones over which we had been passing on our way down the Siang.

At Sinchau my servant and boatmen purchased some fossils, which all agreed in saying came from a waterfall 93 li distant among the hills. Small boys gather them at the foot of the fall and bring them to market to sell for curiosities. They were Brachiopoda, probably of the Devonian period, and from the curved parts near the hinge the Chinese call them "hawks." A Mandarin afterward gave me the same account of them. They come from the limestones already described as resting on grits and slates.

The boundary between the provinces of Kwangsi and Hunan is about 100 li down the Siang from Sinchau. There only low hills border the river, and the valley of the Siang really begins. All the way from Shauking near Canton to this point, the whole country is one mass of hills.

All through the province of Kwangsi I was attended by one or two policemen, who carried a large paper from their Mandarin saying to all his people that I was traveling through their country with the permission of the Imperial Government at Peking, and forbidding any one to oppose or annoy me. When we came to the next city this paper was countersigned by the Mandarin of that place to show that I had reached his city in safety and that those policemen were absolved from any farther responsibility. But all through the province of Hunan I was constantly guarded by one civil Mandarin and one policeman,

and one military Mandarin and from two to four soldiers besides.

In eight days from Sinchau we reached Kiyang, which is situated on the left bank of the river and not some distance back from it as placed on the maps. Below this city the whole country becomes thickly populated and well cultivated, and the temples outside of the cities, which were nearly all destroyed by the Taipings, appear everywhere newly built, contrasting most favorably with the ruinous condition of such buildings in every other part of the empire, and indicating how well the people were prospering by whose contributions they were built. They are so numerous and form such a conspicuous part of every view along the Siang, that Hunan may well be styled the province of temples and the strong hold of Buddhism.

Eight or ten li below Kiyang, on the right bank, strata of limestone were seen resting very unconformably on other limestone strata as if the upper layers belong to an entirely different formation. Nothing of this kind had been seen before. The lower limestones had the jointed and fissured appearance of that previously noticed. Eighty-four li below Kiyang, at the village of Pin-cha-bu, we passed a hill of limestone *interstratified with coal.* They were quarrying the lime rock and using the coal obtained at the same time to burn it to lime. The dip of these strata is 40° to the north. A little farther in that direction came red sandstone with a similar dip of 15° to 20°.

Sept. 16th, stopped for the night in a little village 165 li above Hangchau. As we arrived after dark no one saw me and I was left unmolested. All the evening there was even more loud talking than I had been accustomed to hear, as if most of the village had been indulging rather freely in samshoo. At night we always anchored in the stream, and that evening my servant asked one of the Mandarins if he would like to take a walk along the front street. But he only shrugged his shoulders, shook his head in an ominous manner and said, "they are all the worst of ruffians there!"

About ten o'clock a loud talking and disputing began on the bank near us and soon one man commenced screaming and groaning as if had received his death blow. Immediately his murderers brought him down the bank, put him in a small boat and paddled out by us into the middle of the stream, their victim all this time groaning more and more feebly and evidently dying. My servant who was on the watch then informed me that this man was a merchant and belonged to another village and was taking some money to Hangchau, and when the people there robbed him and he shouted out for the police, they stabbed him and were finishing their work by sinking him in the

river. By this time, after the evil was done, the Mandarins at Yamun began firing small cannon every ten or fifteen minutes; and this they kept up regularly for some two hours, showing plainly to us all that they expected to be attacked next themselves.

I found we had thus unwittingly ran directly into a nest of those assassins who prowl in bands over the whole country. I trusted however that no one had seen me, for that was my only hope of saving my life.

Nothing remained for me to do but keep as quiet as possible and leave the place at the earliest dawn. I therefore sat down quietly, opened the lid of my revolver box—for I believe it is a duty every one owes to his Creator to save his life at any cost until he is convicted of some crime—and coolly determined when the event came to sell my life as dearly as possible. But after listening with the keenest solicitude for many long, lonely hours, I finally fell asleep, and when I awoke again our boat was floating down the stream and this village of assassins was far out of view behind us.

We soon came to Lichang the principal coal mine on the Siang. It is situated on the left bank of the river some 50 miles above Hangchau. The coal beds here were seen resting on limestone, and this is also the case in Sz'chuen, at the coal mines near Peking, and probably in every part of the empire where both occur. On the coal strata rests a red sandstone, which originally covered all these coal deposits, at least, in this region; and the coal appears at the surface only where it has been thrust up through the overlying strata of red sandstone or where this sandstone has suffered very considerable denudation. As we were but six miles from the village where the murder occurred, the Mandarin sent to protect me declared he would not let me go on shore and inspect the mines on any condition, and therefore I could note only what was to be seen from the river as we passed. All the so-called "mines" that thus came in view were nothing more than deep pits in the sides of hills and consequently only "surface coals" have been obtained.

It is probable that the best coal occurs only below the water level, and the Chinese are obliged to stop when they come to that for want of proper pumping apparatus. In support of this view I may add that the engineers on the steamers at Hankow informed me that this coal had improved considerably since they first began to use it. The best coal in China therefore remains undisturbed, but there is much reason, in my opinion, to doubt whether it will ever equal the best coal in England and America. Hangchau is the great coal depot for the pro-

vince of Hunan, and the military Mandarin that accompanied me from that city to Changsha, the capital, stated that it is mined at Kweiyang and Laiyang (see Dr. Williams's map of China) and also at Sinhwa on the Tsz'kiang. It probably occurs almost everywhere beneath the red sandstone that covers the wide plains in the whole province, but it is only mined where it crops out along the borders of those plains.

From Lichang in Hunan to Moukden the capital of Manchuria, there is an almost continued series of coal mines on the flanks of the elevations that form the western border of the Great Plain. This wide distribution of coal promises much for the future development of this land.

The most important place for trade in Hunan is Siangtan, 90 li south of Changsha. All the boats that come down the numerous branches of the Siang make this their point of rendezvous, and there is water enough for small steamers from Hankow, unless a shallow bar occurs where this river empties into Tungting Lake. That place I crossed by night and therefore had no means of satisfying myself on that point. When I reached the lake a heavy northerly wind had been blowing for six or seven days and few or no boats had crossed it during that time. A southerly breeze then set in and all the boats that had been in the many creeks and bays came out on the lake, and at sunrise I enjoyed a view only to be witnessed in this land whose population is numbered by the hundred million. As far as the eye could see before us, behind us and for several miles on either side, the surface of the lake was perfectly feathered with white sails, some in sunshine, some in shadow and some in the dim distance, gliding on a thin film of air over the water. Twice during the day I counted nearly *four hundred and forty* in sight at one time; and with the aid of my field glass fully one hundred more could be distinguished. Many were loaded with tea, many with coal, and many were just swimming along under huge deck loads of round timber. This shows the amount of the carrying trade between Siangtan with Changsha; and Yohchau, and Hankow and other cities down the Yangtse. It also indicates that Siangtan is the most important place up the Yangtse that is not yet open to trade.

Below Yohchau a number of lakes occur, which Père Huc describes as abounding in floating islands, but I did not see *one*, and others who had passed to and fro over most of this area informed me they had never heard of such a thing.

Oct. 5th. After sixty three days of continued traveling I at last reached Hankow—the distance by the route taken being about 1200 miles. For thirty-five days I had lived just as a common coolie, and frequently when we were waiting at the

cities for the Mandarins, I have laid in the bottom of my boat among the cargo with a straw mat over me without daring to stir for nearly half a day, for fear of a repetition of what occurred at Kweilin.

Once I had a severe attack of fever and ague, which seemed to set my brain on fire, and for fear I should lose command of my mind I gave my passport and money to my servant and ordered him to be sure to take care of me if I should become delirious, and to take me along with him to Hankow where my friends would reward him with an ample present. Fortunately, after suffering severely for a few days, I shook off the disease, encouraged by the idea that every hour was bringing me nearer the end of my weary journey. For the last fifteen days I did not once have an opportunity of leaving my boat and walking along the bank, the country was so completely submerged by a flood. The fever again began to burn in my veins but the exciting thought that soon I should be free from my persecutors sustained me until I found myself once more in the house of some American friends. All cause for solicitude was then over and for a week the doctor ordered me to keep my chamber.

This journey was undertaken with the hope of ascertaining the kinds of the rocks in the region traversed and the order of their superposition. The time chosen was the dry season, and admitted by all to be a *very* dry season. In such a country where no artificial sections can be seen, perhaps the river channels and the gorges in the mountains offer as good facilities as can be found, to ascertain the geological structure of a country. I therefore made my journey mostly in boats, which over a large portion of the area were the only means of traveling from place to place. It was only necessary to keep the boat near the bank and the strata could be seen and followed continously mile after mile, and when that series disappeared, those above it or below it could be studied in the same manner. In this way, from actual observation, the series was found to be: *First* and *lowest*, *granite;* on which rests the *second* formation composed of *grits and slates.* I am not aware that any fossils have ever been discovered in these rocks. These grits and slates are covered by the *third formation of old limestones,* which the fossils obtained at Sinchau lead us to regard as probably belonging to the Devonian age. On these rest, *fourthly,* another series of limestone strata of the same geological age as the coal beds. A rare collection of fossil plants of these rocks in the neighborhood of Peking was given me by l'Abbé David. They probably belong to the same geological age as the fossil plants sent by Mr. Pumpelly to Dr. Newberry, who regards them as later than the Carboniferous

period and probably Triassic. My journey through the great coal fields of Hunan also gave me an opportunity of more narrowly defining its limits. The route herein described was the one chosen for a railroad between Canton and the southern parts of the empire, and Hankow and the central parts of the country. But no one had been through the mountainous regions and ascertained whether there was a break in the Meiling Range or whether great tunneling would be necessary. Having passed over the whole area, I am prepared to say that there is no physical feature that would render the construction of such a road a work of any greater difficulty there than in a very hilly land. The great obstacles to such work in every part of China are, first, their bitter hostility to foreigners, and secondly, their superstitious fears that any such work "will affect the winds and rain and deluge their crops with floods or parch them with heat." The prevalence of this belief, and the extent to which it influences all their actions, are most surprising.

All over the empire are seen from place to place, deserted quarries of limestone and sandstone, and lead and other mines excepting coal. On inquiring why they do not continue to work in these places, the invariable reply has been "because it is forbidden by the Emperor." But in pressing this question farther I have found in every case except one that the Imperial edict has been made in this way. When a man commences an excavation, the neighboring community draw up a petition that this man be compelled to stop forthwith, that they may not lose their crops. This petition is handed to the Emperor, who not daring to oppose the will of the majority, in a most condescending manner says let the petition be granted. The work is then ended and thus the Emperor, while he plays the part of an absolute monarch, is really a supple slave. These facts therefore show that there is little or no inducement for either native or foreign capitalists to commence a railroad or any other such work in China until the government will not only give its consent, but also *can* and *will* guarantee to protect such property or fully make up any damage the people may do. When this can be done, it is as certain that railroads will pay there as that native and foreign merchants find it profitable now to use steamers on the Yangtse and Canton rivers and along the sea coast. Then, and not till then, will these great improvements be begun in China, and her future promise to be something more than a mere repetition of the past.

Up to the date of this journey it had been a matter of speculation whether there was a water communication between the river system of the Sikiang and that of the Yangtse. This

query is at once answered by the fact, that if the gentry of Kweilin and the people in the adjoining country had allowed me to proceed at my leisure and had not forced me to fly for my life, I could, even in that remarkably dry season, have performed the whole journey in boats except *nine* miles; and I am confident that if I had left Canton in the rainy season I could have made the whole distance of *two thousand miles*, through the interior of China, and come out to the sea coast again at Shanghai *in one and the same boat.*

This enables us to realize that the next wonder in regard to China, after the density of her population, is the completeness of her internal water communication.

ART. II.—*Preliminary notice of a Scorpion, a Eurypterus? and other fossils, from the Coal-measures of Illinois;* by F. B. MEEK and A. H. WORTHEN.

AMONGST some fossils discovered last summer by Mr. Joseph Even, in the iron nodules of the Coal-measures at Mazon creek, Grundy county, Illinois, and loaned by him for the use of the Illinois Geological Survey, there are a few types of such unusual interest, that we have thought it desirable to present a preliminary notice of them, in advance of more extended descriptions and illustrations, to appear in one of the reports of the Survey.

The first of these is a fine *Eurypterus*, or a species of a closely allied genus, between the size of *E. remipes* and *E. robustus*. The specimen consists of an impression in the matrix, of the under surface of all the thoracic, and a part of one or two of the abdominal segments, in a more or less nearly perfect condition; with the operculum or thoracic flap, the post-oral plate, and the maxillary or basal joints of the swimming feet, all in place. There are also prints of some of the succeeding joints of one of the latter organs, and its oar-like expansion, and obscure impressions of three of the smaller legs on one side, and some of the basal joints of the same on the other side *— all converging to the position of the mouth immediately in front of the post-oral plate. As the carapace and the dorsal portions of the thorax are enveloped in the other half of the concretion, which was not found, and the posterior portions of the abdomen and the telson extended beyond the limits of the concretion, we know nothing of the nature of any of these parts.

* These legs are slender, apparently without lateral spines, and terminate in a single long, acutely pointed dactylus, as in *Pterygotus*.

The post-oral plate is about 0·76 inch in length, and 0 55 inch in breadth, the widest part being very slightly behind the middle. Its general form is subovate. From near, or a little behind the middle, it rounds off rather rapidly to the rounded posterior end, and tapers more gradually to the anterior extremity, which is rounded on each side, and rather distinctly emarginate in the middle. The maxillary joints or plates of the swimming feet expose a subtrigonal outline—their length being 0·85 inch, and their breadth at the posterior margin about 0·70 inch. Their lateral slopes are slightly sinuous along the middle, while their anterior ends (or the portions exposed) are very narrow, pointed, curved inward, and extend scarcely beyond the anterior end of the post-oral plate. The succeeding joints are visible, but scarcely in a condition to be described.

The thorax measures 2·45 inches in breadth near the middle, and a little more than 2 inches in length. Its middle segments (on the ventral side) are 0·35 inch in length or anterio-posterior diameter, while the anterior and posterior ones, especially the latter, are shorter; and they are all rounded on their posterior lateral angles. Some impressions in the matrix, however, show that the lateral terminations of the *dorsal* portion of the posterior thoracic segments or rings extended out beyond the rounded ends of those below into acutely pointed extremities, directed obliquely outward and backward. These projecting points of one of the posterior thoracic segments are seen to extend out obliquely 0·46 inch beyond the rounded terminations of those below, and to terminate in very acute points. A portion of one of the anterior abdominal segments remaining, if not accidentally slipped a little to one side, shows that the abdomen is comparatively narrow, and that its segments have the posterior lateral extremities terminating in strong angular processes, nearly straight behind, but with an oblique anterior margin.

The operculum, or thoracic flap, has its lateral alæ, as in the typical species of *Eurypterus*, looking as if composed of two of the body segments anchylosed, the anterior one being not more than half the breadth (more properly length) of the other, which is of the same size as the body segments.* Its mesial appendage is remarkably long, as it can be traced back beyond the poste-

* These alæ in *E. remipes*, and other Silurian species, resemble body segments so closely that they were actually mistaken by Prof. Hall, for "anchylosed first and second segments of the body" (Palæont. N. Y. vol. 3, p. 398, fig. 3, 1 and 2), and not suspected by him to be homologous with any part of the free operculum itself, of the genus *Limulus*. This error has been pointed out and corrected by Mr. Woodward, in his memoir on the genus *Pterygotus*. published by the Paleontographical Society, pp. 40 and 41—1866.

rior margin of the fifth thoracic segment, or to a length of 1·60 inches, while we are almost positively sure it is not bipartite at the extremity. There is also on each side of the anterior, or attached extremity of the mesial appendage, a small spatulate piece, not corresponding to any of the parts of the operculum of *Eurypterus*, as hitherto illustrated, so far as we have yet seen. These are about 0·41 inch in length, and 0·15 inch in breadth, with nearly parallel sides and pointed anterior extremities, while their posterior ends are transversely truncated, with lateral angles rounded. Their anterior pointed ends terminate nearly in contact with the posterior angles of the two little "intercalated pieces" (*a a*) of Prof. Hall's fig. 3, p. 398, of third vol. Palæont. N. Y.; so that they occupy exactly the position of what are apparently intended in that figure to represent the inner truncated ends of the lateral alæ, immediately on each side of the mesial appendage. They are proportionally wider, however, and extend back slightly beyond the posterior margins of the lateral alæ, and are certainly separate pieces. They were evidently over-lapped, on their inner edges at least, by the mesial appendage, and look as if, in case they were attached at all to the operculum, it must have been to its inner side. Possibly, however, they were really attached to the anterior thoracic segment, and form no part of the operculum itself; in which case they would seem to represent, though greatly smaller, the membranaceous modified feet of Limulus, bearing the branchiæ. As now seen, however, in the condition of impressions, they certainly look as if appendages of the operculum; while they show the same scaly sculpturing seen on other parts of the surface.

All the portions of the under side of the fossil, that have left their impressions in the matrix, were provided with fine subimbricating scale-like markings.

From some of the characters mentioned, particularly the great length and non-bipartite extremity of the mesial appendage of the operculum, and the presence of the two additional spatulate appendages, one on each side of the mesial one, we are very much inclined to believe other characters will be found, separating this species generically, or at least subgenerically, from the typical *Eurypteri*. If so, it may be called *Anthraconectes*. For the present, however, we would designate it as *Eurypterus* (*Anthraconectes*) *Mazonensis*.

It is worthy of note here, that Jordan and von Meyer have proposed the name *Adelopthalmus*, for a type from the Coal-measures of Bohemia, very similar to *Eurypterus*, but differing in having no eyes, at least in the position they occupy in *Eurypterus*; while it also differs in having the lateral extremities

of the dorsal half of each thoracic segment more pointed and projecting apparently farther beyond those below. As our fossil shows the latter character (but with these projecting extremities much more acutely angular), as well as the same sudden contraction in the breadth of the body, behind the sixth or seventh segment, it is barely possible it may be found to belong to the same group, and have to take the name *Adelopthalmus Mazonensis.* As their type, however, shows only the *upper* side, and ours the impression of the *lower* only, we have not the means of settling this question at present.

Another specimen, from the black shale of the Coal-measures of Grundy county, near the Mazon creek locality, but from apparently a little higher horizon, has exactly the form and appearance of one side of the carapace of a large *Ceratiocaris;* to which genus we would have no hesitation in referring it, were it not for its rather solid shelly texture, non-striate surface, and the fact that that genus has not hitherto, we believe, been found in the Carboniferous rocks. It is quite thin, however, of an irregular rhombic subovate form, and nearly as large as *C. acuminatus* (3d vol. N. Y. Palæont. pl. 84, fig. 6), which it resembles in general outline, excepting that its truncated posterior margin is so deeply sinuous as to leave the posterior dorsal extremity above the sinus (as well as that below) rather acutely angular; while its anterior extremity, above the obliquely truncated antero-ventral margin, is obtuse instead of pointed. Its basal margin is also more prominent slightly behind the middle. The surface shows no striæ, or other markings, and the margins are smoothly and sharply defined; that of its posterior sinuous edge being also neatly beveled. It may be called *Ceratiocaris? sinuatus.*

Length, 3·36 inch; height, 2·75 inch.

The specimen of the scorpion alluded to at the head of this notice, consists of a cast and mould of the fossil, as revealed in splitting open a concretion; and shows most of the cephalothorax and mandibles, in a somewhat crushed condition, the dorsal side of the seven abdominal segments, and three of those of the tail, all in place:—also four of the legs on one side, and one of the peculiar comb-like organs, characteristic of the family *Scorpionidæ*, detached and lying in the matrix near one side of the abdomen.

The cephalothorax seems to be sub-quadrangular in form, somewhat wider (behind) than long, the breadth being about 0·45 inch. Unfortunately it is not in a condition to show the ocelli, nor can we see whether or not its anterior edge is emarginate. Its posterior edge has a very slender, minutely crenate, raised marginal line, from a little in advance of which

there originates a distinct mesial furrow, that extends forward to near the middle of the shield, where it is intersected by two oblique furrows, with the prominence for the mesial ocelli between them. Two other deep lateral furrows extend, one on each side, from the posterior end of the mesial one, obliquely outward near the posterior margin. The surface is ornamented with irregular scattering granules, mostly upon the prominences between the furrows. The mandibles are very stout, with the structure of those of the true *Scorpionidæ*, and seem to be without distinct teeth; movable finger curved and sharp at the point. Palpi unknown. Legs stout, long, with long joints gradually decreasing in breadth, and apparently like all the other parts, without hairs, serrations or spines.

The abdomen is a little more than twice the apparent length of the cephalothorax, or about 0·90 in. in length and 0·60 in. wide, and has its segments (on the dorsal side) gradually increasing in their antero-posterior diameter backward to the sixth one, the anterior one being narrowest in the middle, in consequence of the broadly sinuous character of its anterior margin; the first, second and sixth ones have their latter extremities rounded,—the third, fourth and fifth have them rounded in front, and nearly rectangular behind. The seventh or last abdominal segment is twice and a half as long as the sixth or longest of the others (being 0·35 inch long and 0·48 in breadth), subtrigonal in form, with the posterior angle broadly truncated for the attachment of the tail, and the anterior lateral angles narrowly rounded. They all have the surface a little granular, the granules being very small and arranged along near the posterior margins. The last one also shows, on the posterior half of the middle, two longitudinal, parallel rows of rather crowded minute punctures.

Of the tail, only the anterior three segments are preserved in the specimen. These show that it was rather stout, but as distinct from the abdomen, by its sudden contraction in breadth, and the form of its segments, as in the living examples of the *Scorpionidæ*. It segments measure as follows:—first one, 0·26 inch in length and 0·24 in breadth; second, 0·34 in length and 0·22 in breadth; third, 0·37 in length and 0·18 in breadth. They are all oblong in form, more or less nearly rectangular at their ends, and, as near as can be determined from a flattened specimen, apparently provided with three longitudinal ridges, with scattering granules above.

The single detached comb-like organ seen lying in the matrix on one side of the abdomen shows eleven of the little bars of division, but is incomplete at both ends.

Although the discovery of such a type in our Coal-measures, even in the mutilated condition of this specimen, is one of much interest, it is still greatly to be regretted, that its condition is such as to show no traces of the ocelli, neither mesial nor lateral, nor of the palpi and terminal portions of the tail,—since these, especially the ocelli, are the very parts upon which generic distinctions are based by most of the naturalists who have investigated the living Scorpions. Consequently, we are left entirely without the means of deciding which of the known genera it would fall into, if not a new generic type. Its general physiognomy, however, the structure of its mandibles, and particularly the possession of the peculiar comb-like organs, leave no doubt whatever in regard to its belonging to the family *Scorpionidæ*, as defined by the generality of authors.

On comparison with the only other Scorpion known to us from the Carboniferous System—(*Cyclopthalmus Sternbergi* from the Coal-measures of Bohemia)—it will be found to differ remarkably in having its tail as distinct from the abdomen, in form and breadth, as in the modern Scorpions (with which it agrees exactly in general appearance, so far as its parts are yet known), instead of having its abdomen passing imperceptibly into the tail, without any well defined change in the form of the segments.

Although our specimen does not retain the anterior part of the cephalothorax, and therefore shows no traces of the eyes, it is evident they could not have been arranged in a large circle around the central prominence for the mesial pair, as in *Cyclopthalmus*, since the posterior two-thirds of the cephalothorax is well preserved, and would include more than half of the circle of eyes if they were arranged as in that genus.

A comparison with recent Scorpions leads us to think it most nearly resembles, in general appearance, the group of species generally referred to Leach's genus *Buthus*. In size, form, and particularly in the ridges and furrows of the *cephalothorax*, and the stoutness of its tail, it resembles *Buthus hirsutus*, of Wood, from California. From these points of general resemblance, and the necessity for some name by which the fossil can be conveniently referred to, we would propose to designate it provisionally, until specimens can be found showing the generic characters, as *Buthus ? ? carbonarius*. In the meantime, those who, like Gervais and others, do not admit the modern subdivisions of the Linnæan genus *Scorpio*, can, in a general way, call it *Scorpio carbonarius*.

That it will be found strictly identical with any modern genus, however, when all its characters can be made out, is exceedingly improbable; and we are prepared to believe more

nearly perfect specimens will show it to be typical of a new genus. If so, it might be called *Eoscorpius*, in allusion to its early appearance in time.

Amongst the specimens we have, from time to time, had an opportunity to examine from the Mazon creek locality, we have frequently observed impressions of a long, many jointed fossil, in regard to the true nature of which we have been unable to arrive at any very satisfactory conclusion. We could hardly doubt but it was an Articulate of some kind, and were inclined to believe it a Myriapod. All our examinations, however, of the specimens at that time known, failed to quite satisfy us that what looked like legs projecting out into the matrix, were really not rigid, inarticulate spines.* Hence, we were left in some doubt, whether or not it might be the mould of the vertebral column of some little vertebrate animal; and after many careful examinations, we concluded to lay the specimens aside, hoping that better examples might be found, giving some clue to their nature. In the meantime, however, we showed all of them we had seen to several of our most eminent naturalists, none of whom could give us any suggestions in regard to the affinities of the animal.

Several of the specimens now before us, of this same fossil, found last summer by Mr. Even at the same locality, are more complete, in some respect, than any we had previously seen; and from these we are satisfied that it is not a vertebral column, but really an articulated animal. As it seems impossible that it can be a Crustacean, an Annelid, or larval insect, we can scarcely doubt that it is a gigantic *Myriapod.*

One of the specimens now before us seems to be entire, and has apparently a sub-hemispherical head, as wide as any part of the long slender body. It is not in a condition to show the eyes, if any existed, nor can we see any remains of mandibles, antennæ, or other appendages connected with it. The entire length of this specimen is 3·90 inches, and its breadth about 0·20 inch. It tapers very little from the anterior to the posterior end, which terminates rather abruptly. In the whole length, as many as seventy-five or seventy-six segments can be counted; but it is worthy of note, that there are only ***half this number*** on the dorsal side, where each one corresponds to two below.† As seen in a side view, the downward curved ends of the dorsal scutes, if we may so call the larger dorsal portions of the segments, are rounded in outline; while each of them

* At that time, we had only seen specimens showing the dorsal spines distinctly, and no well preserved legs.

† This peculiarity of the segments of the body occurs in some types of existing *Myriapoda*. (Family *Cermatiidæ*, in which the dorsal scutes are also generally spiniferous.)

bears apparently three or four short, pointed, rigid spines, directed obliquely backward, and arranged so as to form as many longitudinal rows along the back and dorso-lateral parts of the animal. Some of these spines are seen to give off a small, short, lateral fork, or branch, on the anterior or posterior side.

On the under side of the body there are, as already stated, two segments, or rather two half rings of the dermal integument, to each one above, and these of course are scarcely half the size of those above, though each bears a pair of small slender jointed legs, about 0·20 inch in length, in the specimen nearly 4 inches long. So far as can be made out, there are at least five gradually tapering joints to each leg. In some of the specimens these lower segments show appearances of something like spiracles, though we are not sure that they are such. For a long time we failed to detect joints in the legs, but in some of the specimens now before us they can be clearly seen.

Under a magnifier, the impressions of the body rings in the matrix show a minutely granular kind of marking, that must have been produced by minute pitting of the surface. No hairs, however, have been seen projecting from any part of the fossil.

For this uncouth looking creature, we would propose the generic name *Euphoberia*, in allusion to the formidable appearance a living example, more than a foot in length, must have presented, when alive and moving about, with its back bristling with forked spines, and its 150 legs in motion. Some fragments in the collection are much larger than the most complete specimen from which our measurements are given, and if of the same proportions, the individuals to which they belonged must have been from 12 to 15 inches in length, and near ¾ of an inch in diameter. The specimens seem to belong to two species, one comparatively small, and one large. For the smaller typical one, from which the foregoing description was made out, we would propose the name *E. armigera*, and for the larger *E. major*.*

On comparing our specimens with a curious jointed, spiniferous fossil, figured and described by Mr. Salter in the Quarterly Journal of the Geological Society of London, vol. xix., p. 84, fig. 8, from the Staffordshire Coal-measures, under the name *Eurypterus ?* (*Arthropleura*) *ferox*, we can scarcely entertain

* Jordan and von Meyer have proposed the name *Chonionotus* (see Palæontographica, vol. 4, 1856, pl. 11, fig. 3) for a fragment of a jointed fossil, from the Coal-measures, that may *possibly* belong to the *under* side of a species of this genus, though it is not very probable that it does. If it does, however, our generic name would have to be abandoned, and the name of our species would become *Chonionotus armiger*, and *C. major*.

a doubt of their generic identity. Indeed, if it were not that his species has the dorso-lateral spines each provided with three instead of two prongs, and proportionally longer, it would be difficult to point out even specific differences. His specimen consists merely of the dorsal side of six of the segments preserving the dorso-lateral rows of spines, and two dorsal rows of tubercles, which we are inclined to think must have also borne spines. Mr. Salter thought it probably a part of the central lobe of a trilobate *Eurypterus*, or some allied genus, an opinion he would not for a moment have entertained (provided we are right in regard to its relations to our fossil) had he seen a specimen showing a side view of even a few segments of the animal. At any rate, our fossil is entirely distinct, in all respects, from the typical species of *Arthropleura* of Jordan and von Meyer, which is almost certainly a Crustacean.

Supplementary Note on some of the Morris Crustacea, &c., formerly described.

A number of additional specimens now before us, in various conditions of preservation, of some of the articulates already described and figured in the Illinois State Geological Report, from the Grundy county locality, enable us to add some facts in regard to these fossils not determinable from the specimens first obtained. This additional information we give below :—

1. Genus *Acanthotelson*. It is a little remarkable, that all the specimens of the two known species of this genus, at first obtained, lie flattened in the concretions on *one side* or the other, as shown in our figures. The fact that so many individuals had been thus found, and none enveloped as if standing with the thoracic legs spread out on each side for walking, led us to think the former probably their natural posture, and that like the typical *Amphipoda*, they might not have had the power of standing upon their legs. Several of those more recently obtained, however, are enveloped in such a manner, that in splitting open the concretions in the plane of their greatest (horizontal) diameter, we have exposed a dorsal view of the fossil, with its thoracic legs extended out on each side, so as to show that the animal could probably stand and walk upon them, and that this may have been its natural attitude. It is also worthy of note, that in all these specimens, the thoracic legs are *all* directed forward, and not a part of them forward and a part backward, as in most of the *Amphipoda*.

These specimens also clearly show that the last joint of each stylet is, as we had conjectured, bipartite, the two terminal

pieces of each being exactly alike, and scarcely distinguishable in size and form from the telson. They likewise appear to show that the stylets do not, as we had supposed, connect with the same segment as the telson, but probably with the next one in advance of it, and that both the stylets and telson are horizontally flattened. Our outline restorations of these parts in this type, on the plate cited, would be corrected, if this suggestion is right, by marking off a short inconspicuous segment, from the anterior end of the telson, as now represented.

One specimen of apparently a new species, nearly allied to *A. Stimpsoni*, has the peduncles of the outer antennæ well preserved, and shows them to be stout, and composed of three joints. Its body is proportionally about one-fourth longer than that of *A. Stimpsoni*, with proportionally larger anterior legs. As seen on the dorsal side, its body is long, narrow and rounded, with parallel sides, excepting a few of the posterior segment, which gradually taper to the telson. For this larger, more elongate species, we would propose the name *A. Eveni*, after Mr. Joseph Even of Morris, who has discovered most of these interesting fossils.

Several of the specimens show that none of the thoracic legs of this genus bifurcate as in the genus *Gampsonyx*.

2. Genus *Palæocaris*. We are now nearly satisfied, from additional specimens, that the caudal appendages and jointed body represented by our figure *5a*, pl. 32, of the Illinois Report, really belong to the same species as the specimen represented by figure 5 of the same plate, as we had supposed.

Since we now know that in *Acanthotelson* the stylets and telson are horizontally flattened, and that the animal had its legs constructed for walking, and, like its antennæ, very similar to those of the type of *Palæocaris*, we think these types more nearly allied to each other, and to *Gampsonyx*, in some characters, than we had supposed, but still quite distinct from the latter type, in not having the legs divided, and in the different characters of their telson and stylets. We hope to be able to illustrate all these points by figures, before long.

Genus *Anthracerpes*. As no fragments of this fossil have been obtained showing any legs, we are inclined to the opinion that it may be an Annelid.

ART. III.—*On the formation of Nitrite of Ammonia;* by O. LOEW, Assistant in the Chemical Department of City College, New York.

MANY years ago, Liebig showed that alloxan, the immediate product of the oxydation of uric acid, yields with ammonia, a substance of a bright red color, murexyd. Ammonia has therefore been used for many years as a good reagent for alloxan, and for uric acid. Of course, therefore, alloxan can be also employed as a good reagent for ammonia; extremely small traces of ammonia indeed, can be found in this way, if necessary precautions be used. In order to have alloxan in a convenient form as a reagent, I dipped strips of Swedish filter-paper in a solution of alloxan and dried them in vacuo, after having put them into dried ether. In preparing the paper at first, I employed for drying it, the water-bath; but always, before it was perfectly dried, it became red. Afterward I dried it in a bell-jar over sulphuric acid; but here *too* remarked, that the ends of the strips became red. This appearance seemed to me extremely singular and remarkable. All the glass vessels, that I used, were perfectly clean; the air in the bell-jar could not possibly contain ammonia, by reason of the contact with the sulphuric acid; and on the other hand I was sure, that the bell-jar was well closed, so that it was impossible for a trace of ammonia gas to enter from the outside. I repeated this experiment in a modified manner, namely:—I put some milligrammes of alloxan in a test-tube and poured some drops of water on them (distilled water I avoided because of its containing ammonia) and placed some strips of Swedish filter paper in the test-tube in such a way that the lower ends dipped into the above mentioned solution of alloxan, and the upper ends projected a little above the mouth of the tube. The test-tube was then set upright in a bottle (for support), and both were placed in a vessel containing sulphuric acid and standing on a glass plate. A bell-jar was put over the whole, taking care that the junction was air-tight. The apparatus was then set in a place where ammonia had never been used. Next day the tips of the projecting ends began to turn red, and the redness increased, until the last trace of water was evaporated out of the test-tube. I made this experiment the third time, taking care as before, that the vessels I used were perfectly clean. The bell-jar and test tube were washed with diluted sulphuric acid, afterward with alcohol, the strips were not touched with the fingers, only a pincers was em-

ployed. At first I did not remark any reddening, but it became visible in an extremely short time, when the apparatus was placed in the sunlight. I repeated this experiment more than once and always found, that—1, this red color appears only at that part of the paper where the evaporation of the water takes place. 2. This discoleration does not increase after all the water is evaporated and absorbed by the sulphuric acid. 3. This phenomenon makes its appearance very quickly in the sunlight, and slowly in diffused daylight.

Now what are the reasons of this phenomenon? Only one explanation suggests itself, and that is given by the discovery of Schoenbein, in regard to the formation of nitrite of ammonia from water and nitrogen. There can be no doubt that this coloration only comes from the nitrite of ammonia, formed by the evaporation of such a small quantity of water. The open end of the test-tube was in contact with the dried air in the bell-jar; therefore, the upper ends of the strips projecting out of the tube were the chief seat of evaporation. The lower ends sucked up the same amount of water as the upper lost. Thus a considerable amount of water evaporated from a very limited space; the generated nitrite of ammonia was therefore also limited to this space, and on the other hand, the solution of alloxan became concentrated here also, forming a crystalline surface on the ends of the paper. In the ordinary evaporation of water, the newly formed nitrite of ammonia is quickly destroyed again, generating water and nitrogen:—$\left.\begin{matrix}NH_4\\NO\end{matrix}\right\}O = 2H_2O + 2N$ (O=16.) But in our case every trace of this salt is quickly fixed by the alloxan, murexyd being formed, which is the cause of this reddening. If one employs a concentrated solution of alloxan, the experiment will not always succeed, because the crystalline surface formed by it will not allow the water to be sucked up, and therefore the place of evaporation changes. On the other hand, the ammonia salt thus formed is mixed with a large amount of alloxan, so that the phenomenon does not become so readily visible. That not only free ammonia can produce murexyd, but also the nitrite, I have proved; many other ammonia salts produce this color, with alloxan. One sees from this experiment that doubtless every drop of water by its evaporation yields a certain quantity of nitrite of ammonia, and further that the direct sunlight accelerates in a great degree this formation. If we consider the amount of carbonic acid and ammonia in the atmosphere, we find the proportion of C : N=300 : 1 (N in the form of ammonia), but if we compare with that, the proportion of these two elements in the body of vegetables, we find on an average

the ratio of 50 : 1. Whence then comes the nitrogen that is required to make up the proper proportion? The ammonia of the soil is not able to give a sufficient answer to this question, but we have here the explanation: *In the direct sunlight, not only the carbon and hydrogen are taken up in a higher degree, but also the nitrogen of the air, the latter being quickly converted into the easily assimilable form of nitrite of ammonia.*

New York, April 1, 1868.

Art. IV.—*Notes on some Algæ from a Californian hot Spring*; by Dr. H. C. Wood, Jr., Professor of Botany in the University of Pennsylvania.

Some time since Prof. Leidy handed me for examination a number of dried Algæ, which he had received from Prof. Seidensticker, by whose sister, Mrs. Partz, they had been gathered in the "Benton Spring," which is situated in the extreme northern point of Owen's Valley, California, 60 miles southwest from the town of Aurora. Afterward a number of similar specimens came to me directly from Mrs. Partz by mail. The subject of life in thermal springs is one of so much general interest, especially in connection with that of spontaneous generation, as to induce me to make a very careful examination of the material and offer the results to the readers of this Journal. In this connection the following extract from a letter of Mrs. Partz to her brother is very relevant:

"I send you a few samples of the singular vegetation developed in the hot springs of our valley. These springs rise from the earth in an area of about 80 square feet, which forms a basin or pond that pours its hot waters into a narrow creek. In the basin are produced the first forms, partly at a temperature 124°–135° F. Gradually in the creek and to a distance of 100 yards from the springs are developed at a temperature of 110°–120° F. the Algæ, some growing to a length of over 2 feet, and looking like bunches of waving hair of the most beautiful green. Below 100° F. these plants cease to grow and give way to a slimy fungus growth, though likewise of a beautiful green, which finally, as the temperature of the water decreases, also disappears. They are very difficult to preserve, being of so soft and pulpy nature as not to bear the least handling, and must be carried in their native hot water to the house, very few at a time, and floated upon paper. After being taken from the water and allowed to cool they become a black pulpy mass. But more strange than the vegetable are the animal organiza-

tions, whose germs, probably through modifications of successive generations, have finally become indigenous to these strange precincts. Mr. Partz and myself saw in the clear waters of the basin a very sprightly spider-like creature running nimbly over the ground, where the water was 124° F., and on another occasion dipped out two tiny red worms."

In regard to the temperatures given and the observations as to the presence of animal life in the thermal waters, Mr. Wm. Gabb of the State Geological Survey states that he has visited the locality, knows Mrs. Partz very well, and that whatever she says may be relied on as accurate.

The color of the dried specimens varies from a very elegant bluish green to a dirty greenish and fuscous brown. After somewhat prolonged soaking, in hot water, the specimens regained apparently their original form and dimensions, and were found to be in very good condition for microscopical study.

The plant in its earliest stages appears to consist simply of cylindrical filaments, which are so small that they are resolved, with some difficulty, into their component cells by a first class $\frac{1}{8}$th objective. Fronds composed entirely of filaments of this description were received. Some of these were marked as "first forms," and as having grown in water at a temperature of 160° F. Probably these were collected immediately over the spot where the heated water bubbled up. At this temperature, if the collection made is to be relied on as a means of judging, the plant does not perfect itself. To the naked eye these "first forms" were simply membranous expansions of a vivid green color, and indefinite size and shape, scarcely as thick as writing paper, with their edges very deeply cut and running out into a long waving hair-like fringe. Other specimens which grew at a much lower temperature exactly simulated those just described, both in general appearance and microscopical characters.

These I believe are the immature plant.

The matured fronds, as obtained by the method of soaking above described, were "gelatinous-membranous," of a dirty greenish or fuscous brown at their bases and bright green at their marginal portions, where they were deeply incised and finally split up into innumerable hair-like processes. Proximally they were one or even two lines in thickness, distally they were scarcely as thick as tissue paper. Their bases were especially gelatinous, sometimes somewhat translucent, and under the microscope were found to have in them only a few distant filaments.

Two sets of filaments were very readily distinguished in the adult plant. The most abundant of these, and that especially

found in the distal portions of the fronds, were composed of uniform cylindrical cells, often enclosed in a gelatinous sheath. The diameter of such filaments varies greatly; in the larger the sheaths are generally apparent, in the smaller they are frequently indistinguishable.

In certain places these filaments run more or less parallel side by side, and are glued together into a sort of membrane. It is only in these cylindrical filaments that I have been able to detect heterocysts, which are not very different from the other cells. They are about one-third or one-half broader, and are not vesicular but have contents similar to those of the other cells. In one instance only was I able to detect hairs upon these heterocysts.

The larger filaments are found especially near the base and in the other older portions of the frond. Their cells are generally irregularly elliptical or globose, rarely are they cylindrical.

They are mostly of an orange brown color, and there exists a particular gelatinous coating to each cell rather than a common gelatinous sheath to the filament. These larger threads are apparently produced from the smaller filaments by a process of growth.

Near the base and in the under portions of the fronds, these filaments are scattered in the homogeneous jelly, in which they run infinitely diverse courses. In the upper portions of the frond and at some little distance from the base, the adjoining cells are very close to one another and pursue more or less parallel courses, with enough firm jelly between to unite them into a sort of membrane.

This plant certainly belongs to the Nostochaceæ, and seems a sort of connecting link between the genera Hormosiphon of Kützing and Nostoc.

The best algologists now refuse to recognize the former group as generically distinct, and the characters presented by this plant seem to corroborate that view.

The species appears to be an undescribed one, and I would propose for it the specific name *Caladarium*, which is suggested by its place of growth. There are several species of allied genera, which grow in the hot springs of Europe, but no true Nostoc has, I believe, before been found in thermal waters. The following is the technical description of the species:

N. caladarium, sp. nov.—N. thallo maximo, indefinite expanso, aut membranaceo-coriaceo vel membranaceo-gelatinoso vel membranaceo, aut læte viride vel sordide olivaceo-viride vel olivaceo-brunneo, irregulariter profunde laciniato-sinuato, ultimo eleganter laciniato; trichomatibus inæqualibus, inter-

dum flexuoso-curvatis, plerumque subrectis et arcte conjunctis, in formis duabus occurrentibus; forma altera parve, viride, articulis cylindricis, cum cellulis perdurantibus hic illic interjectis, vaginis interdum obsoletis, sæpius diffluentibus, instructa; forma altera maxima, articulis globosis vel oblongis, aurantiaco-brunneis, cellulis perdurantibus ab ceteris haud diversis.

Diam. Cellulæ cylindricæ maximæ $\frac{1}{10000}$ unc. Cellulæ perdurantes $\frac{1}{5000}$ unc.

Diam. Formæ primæ articuli maximi $\frac{1}{10000}$ unc. Cellulæ perdurantes $\frac{1}{5000}$ unc. Formæ secundæ articuli oblongi longi $\frac{1}{2000}$–$\frac{1}{3000}$ unc., lati $\frac{1}{5000}$–$\frac{1}{6500}$, articuli globosi $\frac{1}{3500}$–$\frac{1}{4000}$ unc.

Adherent to, and often more or less imbedded in, the fronds of the Nostoc, were scattered frustules of several species of diatoms, none of which was I able to identify. In some of the fronds there were numerous unicellular Algæ, all of them representatives of a single species belonging to the genus *Chroococcus Nägeli*. This genus contains the very lowest known organisms—simple cells without nuclei, multiplying, as far as known, only by cell division. These cells are found single or associated in small families, and in certain species these families are united to form a sort of indeterminate gelatinous stratum. In this species the families are composed of but very few cells, surrounded by a very large, more or less globular or elliptical mass of transparent, firm jelly. The species is very closely allied to *Chroococcus turgidus*, var. *thermalis* Rabenh, from which it differs in the outer jelly not being lamellated.

The following is the technical description of the species:

C. thermophilus, sp. nov.—Ch. cellulis singulis, aut geminis vel quadrigeminis et in familias consociatis, oblongis vel subglobosis, interdum angulosis, haud strato mucoso formantibus; tegumento crassissimo, achroo, haud lamelloso, homogeneo; cytioplasmate viride, interdum subtiliter granulato, interdum homogeneo.

Diam. Cellulæ singulæ sine tegumento longitudo maxima $\frac{1}{1500}''$, latitudo maxima $\frac{1}{2300}''$.

Art. VI.—*On Faraday as a Discoverer;* by John Tyndall, F.R.S.*

Parentage: Introduction to the Royal Institution: Earliest Experiments: First Royal Society Paper: Marriage.

It has been thought desirable to give you and the world some image of Michael Faraday, as a scientific investigator and discoverer. The attempt to respond to this desire has

* From the Report of the Royal Institution of Great Britain.

been to me a labor of difficulty, if also a labor of love. For however well acquainted I may be with the researches and discoveries of that great master,—however numerous the illustrations which occur to me of the loftiness of Faraday's character and the beauty of his life,—still to grasp him and his researches as a whole; to seize upon the ideas which guided him, and connected them; to gain entrance into that strong and active brain, and read from it the riddle of the world—this is a work not easy of performance, and all but impossible amid the distraction of duties of another kind. That I should at one period or another speak to you regarding Faraday and his work, is natural, if not inevitable; but I did not expect to be called upon to speak so soon. Still the bare suggestion that this is the fit and proper time for speech sent me immediately to my task: from it I have returned with such results as I could gather, and also with the wish that those results were more worthy than they are of the greatness of my theme.

It is not my intention to lay before you a *life* of Faraday in the ordinary acceptation of the term. The duty I have to perform is to give you some notion of what he has done in the world; dwelling incidentally on the spirit in which his work was executed, and introducing such personal traits as may be necessary to the completion of your picture of the *philosopher*, though by no means adequate to give you a complete idea of the *man*.

The newspapers have already informed you that Michael Faraday was born at Newington Butts, on the 22nd of September, 1791, and that he fell finally asleep at Hampton Court, on the 25th of August, 1867. Believing as I do, in the general truth of the doctrine of hereditary transmission—sharing the opinion of Mr. Carlyle that "a really able man never proceeded from entirely stupid parents"—I once used the privilege of my intimacy with Mr. Faraday to ask him whether his parents showed any signs of unusual ability. He could remember none. His father, I believe, was a great sufferer during the latter years of his life, and this might have masked whatever intellectual power he possessed. When thirteen years old, that is to say in 1804, Faraday was apprenticed to a bookseller and bookbinder in Blandford street, Manchester-square: here he spent eight years of his life, after which he worked as a journeyman elsewhere.

You have also heard the account of Faraday's first contact with the Royal Institution: that he was introduced by one of the members to Sir Humphry Davy's last lectures; that he took notes of those lectures, wrote them fairly out, and sent them to Davy, entreating him at the same time to enable him

to quit trade, which he detested, and to pursue science, which he loved. Davy was helpful to the young man, and this should never be forgotten: he at once wrote to Faraday, and afterward, when an opportunity occurred, made him his assistant.* Mr. Gassiot has lately favored me with the following reminiscence of this time:—

"CLAPHAM COMMON, SURREY,
"28*th November*, 1867.

"*My dear Tyndall*,—Sir H. Davy was accustomed to call on the late Mr. Pepys in the Poultry on his way to the London Institution, of which Pepys was one of the original managers; the latter told me that on one occasion, Sir H. Davy, showing him a letter said, 'Pepys, what am I *to do*, here is a letter from a young man named Faraday; he has been attending my lectures and wants me to give him employment at the Royal Institution, *what can I do?*' '*Do*,' replied Pepys, 'put him to wash bottles; if he is good for anything, he will do it directly; if he refuses, he is good for nothing.' 'No, no,' replied Davy; 'we must try him with something better than that.' The result was, that Davy engaged him to assist in the Laboratory at *weekly* wages.

"Davy held the joint office of Professor of Chemistry, and Director of the Laboratory; he ultimately gave up the former to the late Professor Brande, but he insisted that Faraday should be appointed Director of the Laboratory, and, as Faraday told me, this enabled him on subsequent occasions to hold a definite position in the Institution, in which he was always supported by Davy. I believe he held that office to the last.

"Believe me, my dear Tyndall, yours truly,
"Dr. Tyndall." "J. P. GASSIOT.

From a letter written by Faraday himself soon after his appointment as Davy's assistant, I extract the following account of his introduction to the Royal Institution:—

"LONDON, *Sept.* 13*th*, 1813.

"As for myself I am absent (from home) nearly day and night except occasional calls, and it is likely shall shortly be absent entirely, but this (having nothing more to say and at the request of my mother) I will explain to you. I was formerly a bookseller

* Here is Davy's reommendation of Faraday, presented to the managers of the Royal Institution, at a meeting on the 18th of March, 1813, Charles Hatchett, Esq., in the chair:—

"Sir Humphry Davy has the honor to inform the managers that he has found a person who is desirous to occupy the situation in the Institution lately filled by William Payne. His name is Michael Faraday. He is a youth of twenty-two years of age. As far as Sir H. Davy has been able to observe or ascertain, he appears well fitted for the situation. His habits seem good; his disposition active and cheerful, and his manner intelligent. He is willing to engage himself on the same terms as given to Mr. Payne at the time of quitting the Institution.

"*Resolved*,—That Michael Faraday be engaged to fill the situation lately occupied by Mr. Payne, on the same terms."

and binder, but am now turned philosopher,* which happened thus:—Whilst an apprentice, I, for amusement, learnt a little chemistry and other parts of philosophy, and felt an eager desire to proceed in that way further. After being a journeyman for six months under a disagreeable master, I gave up my buisness, and through the interest of a Sir H. Davy, filled the situation of chemical assistant to the Royal Institution of Great Britain, in which office I now remain; and where I am constantly employed in observing the works of nature, and tracing the manner in which she directs the order and arrangement of the world. I have lately had proposals made to me by Sir Humphry Davy, to accompany him in his travels through Europe and Asia as philosophical assistant. If I go at all I expect it will be in October next—about the end, and my absence from home will perhaps be as long as three years. But as yet all is uncertain."

This account is supplemented by the following letter, written by Faraday to his friend De la Rive,† on the occasion of the death of Mrs. Marcet. The letter is dated 2d Sept., 1858:

"*My dear Friend,*—Your subject interested me deeply every way; for Mrs. Marcet was a good friend to me, as she must have been to many of the human race. I entered the shop of a bookseller and bookbinder at the age of 13, in the year 1804, remained there eight years, and during the chief part of the time bound books. Now it was in those books, in the hours after work, that I found the beginning of my philosophy. There were two that especially helped me, the 'Encyclopædia Britannica,' from which I gained my first notions of electricity, and Mrs. Marcet's 'Conversations on Chemistry,' which gave me my foundation in that science. . . . .

"Do not suppose that I was a very deep thinker, or was marked as a precocious person. I was a very lively, imaginative person, and could believe in the 'Arabian Nights' as easily as in the 'Encyclopædia.' But facts were important to me, and saved me. I could trust a fact, and always cross-examined an assertion. So when I questioned Mrs. Marcet's book by such little experiments as I could find means to perform, and found it true to the facts as I could understand them, I felt that I had got hold of an anchor in chemical knowledge, and clung fast to it. Thence my deep veneration for Mrs. Marcet—first, as one who had conferred great personal good and pleasure on me; and then as one able to convey the truth and principle of those boundless fields of knowledge which concern natural things, to the young, untaught, and inquiring mind.

"You may imagine my delight when I came to know Mrs. Marcet personally; how often I cast my thoughts backward, delighting to connect the past and the present; how often, when sending

* Faraday loved this word and employed it to the last; he had an intense dislike to the modern term *physicist*.

† To whom I am indebted for a copy of the original letter.

a paper to her as a thank-offering, I thought of my first instructress, and such like thoughts will remain with me.

"I have some such thoughts even as regards *your own father*; who was, I may say, the first who personally at Geneva, and afterward by correspondence, encouraged, and by that sustained me."

Twelve or thirteen years ago Mr. Faraday and myself quitted the Institution one evening together, to pay a visit in Baker street. He took my arm at the door, and, pressing it to his side in his warm genial way, said, "Come, Tyndall, I will now show you something that will interest you." We walked northward, passed the house of Mr. Babbage, which drew forth a reference to the famous evening parties once assembled there. We reached Blandford street and after a little looking about, he paused before a stationer's shop, and then went in. On entering the shop, his usual animation seemed doubled; he looked rapidly at everything it contained. To the left on entering was a door, through which he looked down into a little room, with a window in front facing Blandford street. Drawing me toward him, he said eagerly, "Look there, Tyndall; that was my working-place. I bound books in that little nook." A respectable-looking woman stood behind the counter: his conversation with me was too low to be heard by her, and he now turned to the counter to buy some cards as an excuse for our being there. He asked the woman her name—her predecessor's name—his predecessor's name. "That won't do," he said, with good-humored impatience, who was *his* predecessor?" "Mr. Riebau," she replied, and immediately added, as if suddenly recollecting herself, "He, sir, was the master of Sir Charles Faraday." "Nonsense!" he responded, "there is no such person." Great was her delight when I told her the name of her visitor; but she assured me that as soon as she saw him running about the shop, she felt—though she did not know why—that it must "Sir Charles Faraday."

Faraday did, as you know, accompany Davy to Rome; he was re-engaged by the managers of the Royal Institution on the 15th of May, 1815. Here he made rapid progress in chemistry, and after a time was entrusted with easy analyses by Davy. In those days the Royal Institution published 'The Quarterly Journal of Science,' the precursor of our own 'Proceedings.' Faraday's first contribution to science appeared in that journal in 1816. It was an analyses of some caustic lime from Tuscany, which had been sent to Davy by the Duchess of Montrose. Between this period and 1818 various notes and papers were published by Faraday. In 1818 he experimented upon "Sounding Flames." Professor Auguste De la Rive, father of our present excellent De la Rive, had investigated

those sounding flames and had applied to them an explanation which completely accounted for a class of sounds discovered by De la Rive himself. By a few simple and conclusive experiments Faraday proved that the explanation was insufficient. It is an epoch in the life of a young man when he finds himself correcting a person of eminence, and in Faraday's case, where its effect was to develop a modest self-trust, such an event could not fail to act profitably.

From time to time between 1818 and 1820 Faraday published scientific notes and notices of minor weight. At this time he was acquiring, not producing; working hard for his master and storing and strengthening his own mind. He assisted Mr. Brande in his lectures, and so quietly, skilfully, and modestly was his work done, that Mr. Brande's vocation at the time was pronounced "lecturing on velvet." In 1820 Faraday published a chemical paper "on two new compounds of chlorine and carbon, and on a new compound of iodine, carbon, and hydrogen." This paper was read before the Royal Society on the 21st of Dec. 1820, and it was the first of his that was honored with a place in the 'Philosophical Transactions.'

On the 12th of June, 1821, he married, and obtained leave to bring his young wife into his rooms at the Royal Institution. There for forty-six years they lived together, occupying the suite of apartments which had been previously in the successive occupancy of Young, Davy, and Brande. At the time of her marriage Mrs. Faraday was twenty-one years of age, he being nearly thirty. Regarding this marriage I will at present limit myself to quoting an entry written in Faraday's own hand in his book of diplomas, which caught my eye while in his company some years ago. It ran thus:—

"*25th January*, 1847.

"Amongst these records and events, I here insert the date of one which, as a source of honor and happiness, far exceeds all the rest. We were *married* on the 12th of June, 1821.

"M. Faraday."

Then follows the copy of the minutes, dated 21st May, 1821, which gave him additional rooms, and thus enabled him to bring his wife to the Royal Institution. A feature of Faraday's character which I have often noticed makes itself apparent in this entry. In his relations to his wife he added *chivalry* to affection.

Early Researches: Magnetic Rotations; Liquefaction of Gases: Heavy Glass: Charles Anderson: Contributions to Physics.

Oersted, in 1820, discovered the action of a voltaic current on a magnetic needle; and immediately afterward the splen-

did intellect of Ampère succeeded in showing that every magnetic phenomenon then known might be reduced to the mutual action of electric currents. The subject occupied all men's thoughts; and in this country Dr. Wollaston sought to convert the deflection of the needle by the current into a permanent *rotation* of the needle round the current. He also hoped to produce the reciprocal effect of causing a current to rotate round a magnet. In the early part of 1821 Wollaston attempted to realize this idea in the presence of Sir Humphry Davy in the laboratory of the Royal Institution. This was well calculated to attract Faraday's attention to the subject. He read much about it; and in the months of July, August, and September he wrote "a history of the progress of electro-magnetism," which he published in Thomson's 'Annals of Philosophy.' Soon afterward he took up the subject of "Magnetic Rotations," and on the morning of Christmas day, 1821, he called his wife to witness for the first time the revolution of a magnetic needle round an electric current. Incidental to the "historic sketch" he repeated almost all the experiments there referred to; and these, added to his own subsequent work, made him practical master of all that was then known regarding the voltaic current. In 1821 he also touched upon a subject which subsequently received his closer attention—the vaporization of mercury at common temperatures; and immediately afterward conducted, in company with Mr. Stodart, experiments on the alloys of steel. He was accustomed in after years to present to his friends razors formed from one of the alloys then discovered.

During Faraday's hours of liberty from other duties he took up subjects of inquiry for himself; and in the spring of 1823, thus self-prompted, he began the examination of a substance which had long been regarded as the chemical element chlorine, in a solid form, but which Sir Humphry Davy, in 1810, had proved to be a hydrate of chlorine, that is, a compound of chlorine and water. Faraday first analyzed this hydrate, and wrote out an account of its composition. This account was looked over by Davy, who suggested the heating of the hydrate under pressure in a sealed glass tube. This was done. The hydrate fused at a blood-heat, the tube became filled with a yellow atmosphere and was found to contain two liquid substances. Dr. Paris happened to enter the laboratory while Faraday was at work. Seeing the oily liquid in his tube he rallied the young chemist for his carelessness in employing soiled vessels. On filing off the end of the tube its contents exploded and the oily matter vanished. Early next morning Dr. Paris received the following note:—

"*Dear Sir*—The *oil* you noticed yesterday turns out to be liquid chlorine. "Yours faithfully,

"M. Faraday."*

The gas had been liquefied by its own pressure. Faraday then tried compression with a syringe, and succeeded thus in liquefying the gas.

To the published account of this experiment Davy added the following note:—"In desiring Mr. Faraday to expose the hydrate of chlorine in a closed glass tube, it occurred to me that one of three things would happen: that it would become fluid as a hydrate: that decomposition of water would occur; . . . . or that the chlorine would separate in a fluid state." Davy, moreover, immediately applied the method of self-compressing atmospheres to the liquefaction of muriatic gas. Faraday continued the experiments and succeeded in reducing a number of gases till then deemed permanent to the liquid condition. In 1844 he returned to the subject, and considerably expanded its limits. These important investigations established the fact that gases are but the vapors of liquids possessing a very low boiling-point, and gave a sure basis to our views of molecular aggregation. The account of the first investigation was read before the Royal Society on the 10th of April, 1823, and was published, in Faraday's name, in the 'Philosophical Transactions.' The second memoir was sent to the Royal Society on the 19th of December, 1844. I may add that while he was conducting his first experiments on the liquefaction of gases, thirteen pieces of glass were on one occasion driven by an explosion into Faraday's eye.

Some small notices and papers, including the observation that glass readily changes color in sunlight, follow here. In 1825 and 1826 Faraday published papers in the 'Philosophical Transactions' on "new compounds of carbon and hydrogen," and on "sulphonaphthalic acid." In the former of these papers he announced the discovery of benzol, which, in the hands of modern chemists, has become the foundation of our splendid aniline dyes. But he swerved incessantly from chemistry into physics; and in 1826 we find him engaged in investigating the limits of vaporization, and showing, by exceedingly strong and apparently conclusive arguments, that even in the case of mercury such a limit exists; much more he conceived it to be certain that our atmosphere does not contain the vapor of the fixed constituents of the earth's crust. This question, I may say, is likely to remain an open one. Mr. Rankine, for example, has lately drawn attention to the odor of certain metals; whence comes this odor, if it be not from the vapor of the metal?

* Paris: 'Life of Davy,' p. 391.

In 1825 Faraday became a member of a committee, to which Sir John Herschel and Mr. Dollond also belonged, appointed by the Royal Society to examine, and if possible improve, the manufacture of glass for optical purposes. Their experiments continued till 1829, when the account of them constituted the subject of a "Bakerian Lecture." This lectureship, founded in 1774 by Henry Baker, Esq., of the Strand, London, provides that every year a lecture shall be given before the Royal Society, the sum of four pounds being paid to the lecturer. The Bakerian Lecture, however, has long since passed from the region of pay to that of honor, papers of mark only being chosen for it by the council of the Society. Faraday's first Bakerian Lecture, "On the Manufacture of Glass for Optical Purposes," was delivered at the close of 1829. It is a most elaborate and conscientious description of processes, precautions, and results: the details were so exact and so minute, and the paper consequently so long, that three successive sittings of the Royal Society were taken up by the delivery of the lecture.* This glass did not turn out to be of important practical use, but it happened afterward to be the foundation of two of Faraday's greatest discoveries.†

The experiments here referred to, were commenced at the Falcon Glass Works, on the premises of Messrs. Green and Pellatt, but Faraday could not conveniently attend to them there. In 1827 therefore a furnace was erected in the yard of the Royal Institution; and it was at this time, and with a view of assisting him at the furnace, that Faraday engaged Sergeant Anderson, of the Royal Artillery, the respectable, truthful, and altogether trustworthy man whose appearance here is so fresh in our memories. Anderson continued to be the reverential helper of Faraday and the faithful servant of this Institution for nearly forty years.‡

* *Viz.* November 19, December 3 and 10.

† I make the following extract from a letter from Sir John Herschel, written to me from Collingwood, on the 3rd of November, 1867:—

"I will take this opportunity to mention that I believe myself to have originated the suggestion of the employment of borate of lead for *optical* purposes. It was somewhere in the year 1822, as well as I can recollect, that I mentioned it to Sir James (then Mr.) South; and, in consequence, the trial was made in his laboratory in Blackman street, by precipitating and working a large quantity of borate of lead, and fusing it under a muffle in a porcelain evaporating dish. A very limpid (though slightly yellow) glass resulted, the refractive index 1·866! (which you will find set down in my table of refractive indices in my article 'Light,' 'Encyclopædia Metropolitana'). It was, however, too soft for optical use as an object glass. This Faraday overcame at least to a considerable degree, by the introduction of silica."

‡ Regarding Anderson, Faraday writes thus in 1845:—"I cannot resist the occasion that is thus offered to me of mentioning the name of Mr. Anderson, who came to me as an assistant in the glass experiments, and has remained ever since in the laboratory of the Royal Institution. He assisted me in all the researches into which I have entered since that time; and to his care, steadiness, exactitude, and faithfulness in the performance of all that has been committed to his charge, I am much indebted.—M. F."—*Exp. Researches*, vol. iii, p. 3, footnote.

In 1831 Faraday published a paper "On a peculiar class of Optical Deceptions," to which I believe the beautiful optical toy called the Chromatrope owes its origin. In the same year he published a paper on Vibrating Surfaces, in which he solved an acoustical problem which, though of extreme simplicity *when solved*, appears to have baffled many eminent men. The problem was to account for the fact that light bodies, such as the seed of lycopodium, collected at the vibrating parts of sounding plates, while sand ran to the nodal lines. Faraday showed that the light bodies were entangled in the little whirlwinds formed in the air over the places of vibration, and through which the heavier sand was readily projected. Faraday's resources as an experimentalist were so wonderful, and his delight in experiment was so great, that he sometimes almost ran into excess in this direction. I have heard him say that this paper on vibrating surfaces was too heavily laden with experiments.

Discovery of Magneto-electricity: Explanation of Arago's Magnetism of Rotation: Terrestrial Magneto-electric Induction: The Extra Current.

The work thus far referred to, though sufficient of itself to secure no mean scientific reputation, forms but the vestibule of Faraday's achievements. He had been engaged within these walls for eighteen years.* During part of the time he had drunk in knowledge from Davy, and during the remainder he continually exercised his capacity for independent inquiry. In 1831 we have him at the climax of his intellectual strength, forty years of age, stored with knowledge and full of original power. Through reading, lecturing, and experimenting, he had become thoroughly familiar with electrical science: he saw where light was needed and expansion possible. The phenomena of ordinary electric induction belonged, as it were, to the alphabet of his knowledge; he knew that under ordinary circumstances the presence of an electrified body was sufficient to excite, by induction, an unelectrified body. He knew that the wire which carried an electric current was an electrified body, and still that all attempts had failed to make it excite in other wires a state similar to its own.

What was the reason of this failure? Faraday never could work from the experiments of others, however clearly described. He knew well that from every experiment issued a kind of radiation, luminous in different degrees to different minds, and he hardly trusted himself to reason upon an experiment that he had not seen. In the autumn of 1831 he began to repeat

* He used to say that it required twenty years of work to make a *man* in Physical Science; the previous period being one of *infancy*.

the experiments with electric currents, which, up to that time, had produced no positive result. And here, for the sake of younger inquirers, if not for the sake of us all, it is worth while to dwell for a moment on a power which Faraday possessed in an extraordinary degree. He united vast strength with perfect flexibility. His momentum was that of a river which combines weight and directness with the ability to yield to the flexures of its bed. The intentness of his vision in any direction did not apparently diminish his power of perception in other directions; and when he attacked a subject, expecting results, he had the faculty of keeping his mind alert, so that results different from those which he expected should not escape him through preoccupation.

He began his experiments "on the induction of electric currents" by composing a helix of two insulated wires, which were wound side by side round the same wooden cylinder. One of these wires he connected with a voltaic battery of ten cells, and the other with a sensitive galvanometer. When connection with the battery was made, and while the current flowed, no effect whatever was observed at the galvanometer. But he never accepted an experimental result, until he had applied to it the utmost power at his command. He raised his battery from 10 cells to 120 cells, but without avail. The current flowed calmly through the battery wire without producing, during its flow, any sensible result upon the galvanometer.

"During its flow," and this was the time when an effect wa expected—but here Faraday's power of lateral vision, separating, as it were, from the line of expectation, came into play—he noticed that a feeble movement of the needle always occurred at the moment when he made contact with the battery; that the needle would afterward return to its former position and remain quietly there, unaffected by the *flowing* current. At the moment, however, when the circuit was interupted the needle again moved, and in a direction opposed to that observed on the completion of the circuit.

This result and others of a similar kind led him to the conclusion "that the battery current through the one wire did in reality induce a similar current through the other; but that it continued for an instant only, and partook more of the nature of the electric wave from a common Leyden jar than of the current from a voltaic battery." The momentary currents thus generated were called *induced currents*, while the current which generated them was called the *inducing* current. It was immediately proved that the current generated at making the circuit was always opposed in direction to its generator, while that developed on the rupture of the circuit coincided in direc-

tion with the inducing current. It appeared as if the current on its first rush through the primary wire sought a purchase in the secondary one, and, by a kind of kick, impelled backward through the latter an electric wave, which subsided as soon as the primary current was fully established.

Faraday for a time believed that the secondary wire, though quiescent when the primary current had been once established, was not in its natural condition, its return to that condition being declared by the current observed at breaking the circuit. He called this hypothetical state of the wire the *electro-tonic state:* he afterward abandoned this hypothesis, but seemed to return to it in later life. The term electro-tonic is also preserved by Professor Du Bois Reymond to express a certain electric condition of the nerves, and Professor Clerk Maxwell has ably defined and illustrated the hypothesis in the tenth volume of the 'Transactions of the Cambridge Philosophical Society.'

The mere approach of a wire forming a closed curve to a second wire through which a voltaic current flowed was then shown by Faraday to be sufficient to arouse in the neutral wire an induced current, opposed in direction to the inducing current; the withdrawal of the wire also generated a current having the same direction as the inducing current; those currents existed only during the time of approach or withdrawal, and when neither the primary nor the secondary wire was in motion, no matter how close their proximity might be, no induced current was generated.

Faraday has been called a purely inductive philosopher. A great deal of nonsense is, I fear, uttered in this land of England about induction and deduction. Some profess to befriend the one, some the other, while the real vocation of an investigator, like Faraday, consists in the incessant marriage of both. He was at this time full of the theory of Ampère, and it cannot be doubted that numbers of his experiments were executed merely to test his deductions from that theory. Starting from the discovery of Oersted, the celebrated French philosopher had shown that all the phenomena of magnetism then known might be reduced to the mutual attractions and repulsions of electric currents. Magnetism had been produced from electricity, and Faraday, who all his life long entertained a strong belief in such reciprocal actions, now attempted to effect the evolution of electricity from magnetism. Round a welded iron ring he placed two distinct coils of covered wire, causing the coils to occupy opposite halves of the ring. Connecting the ends of one of the coils with a galvanometer, he found that the moment the ring was magnetized by sending a current through *the other coil,* the galvanometer needle whirled round four or

five times in succession, the action, as before, was that of a pulse which vanished immediately. On interrupting the circuit, a whirl of the needle in the opposite direction occurred. It was only during the time of magnetization or demagnetization that these effects were produced. The induced currents declared a *change* of condition only, and they vanished the moment the act of magnetization or demagnetization was complete.

The effects obtained with the welded ring were also obtained with straight bars of iron. Whether the bars were magnetized by the electric current, or were excited by the contact of permanent steel magnets, induced currents were always generated during the rise and during the subsidence of the magnetism. The use of iron was then abandoned, and the same effects obtained by merely thrusting a permanent steel magnet into a coil of wire. A rush of electricity through the coil accompanied the insertion of the magnet; an equal rush in the opposite direction accompanied its withdrawal. The precision with which Faraday describes these results, and the completeness with which he defines the boundaries of his facts, are wonderful. The magnet, for example, must not be passed quite through the coil, but only half through, for if passed wholly through, the needle is stopped as by a blow, and then he shows how this blow results from a reversal of the electric wave in the helix. He next operated with the powerful permanent magnet of the Royal Society, and obtained, with it, in an exalted degree, all the foregoing phenomena.

And now he turned the light of these discoveries upon the darkest physical phenomena of that day. Arago had discovered in 1824, that a disk of non-magnetic metal had the power of bringing a vibrating magnetic needle suspended over it rapidly to rest; and that on causing the disk to rotate the magnetic needle rotated along with it. When both were quiescent, there was not the slightest measurable attraction or repulsion exerted between the needle and the disk; still when in motion the disk was competent to drag after it not only a light needle but a heavy magnet. The question had been probed and investigated with admirable skill by both Arago and Ampère, and Poisson had published a theoretic memoir on the subject; but no cause could be assigned for so extraordinary an action. It had also been examined in this country by two celebrated men, Mr. Babbage and Sir John Herschel; but it still remained a mystery. Faraday always recommended the suspension of judgment in cases of doubt. "I have always admired," he says, "the prudence and philosophical reserve shown by M. Arago in resisting the temptation to give a theory of the effect he had discovered, so long as he could not devise one which was perfect in its ap-

plication, and in refusing to assent to the imperfect theories of others." Now, however, the time for theory had come. Faraday saw mentally the rotating disk under the operation of the magnet flooded with his induced currents; and from the known laws of interaction between currents and magnets he hoped to deduce the motion observed by Arago. That hope he realized, showing by actual experiment that when his disk rotated currents passed through it, their position and direction being such as must, in accordance with the established laws of electromagnetic action, produce the observed rotation.

Introducing the edge of his disk between the poles of the large horseshoe magnet of the Royal Society, and connecting the axis and the edge of the disk, each by a wire with a galvanometer, he obtained when the disk was turned round a constant flow of electricity. The direction of the current was determined by the direction of the motion, the current being reversed when the rotation was reversed. He now states the law which rules the production of currents in both disks and wires, and in so doing uses for the first time a phrase which has since become famous. When iron filings are scattered over a magnet, the particles of iron arrange themselves in certain determinate lines called magnetic curves. In 1831, Faraday for the first time called these curves "lines of magnetic force;" and he showed that to produce induced currents neither approach to nor withdrawal from a magnetic source, or center, or pole, was essential, but that it was only necessary to cut appropriately the lines of magnetic force. Faraday's first paper on magneto-electric induction, which I have here endeavored to condense, was read before the Royal Society on the 24th of November, 1831.

On the 12th of January, 1832, he communicated to the Royal Society a second paper on Terrestrial Magneto-electric Induction, which was chosen as the Bakerian Lecture for the year. He placed a bar of iron in a coil of wire, and lifting the bar into the direction of the dipping needle, he excited by this action a current in the coil. On reversing the bar, a current in the opposite direction rushed through the wire. The same effect was produced, when, on holding the helix in the line of dip, a bar of iron was thrust into it. Here, however, the earth acted on the coil through the intermediation of the bar of iron. He abandoned the bar and simply set a copper-plate spinning in a horizontal plane; he knew that the earth's lines of magnetic force then crossed the plate at an angle of about 70°. When the plate spun round, the lines of force were intersected and induced currents generated, which produced their proper effect when carried from the plate to the galvanometer. "When the plate was in the magnetic meridian, or in any other plane

coinciding with the magnetic dip, then its rotation produced no effect upon the galvanometer."

At the suggestion of a mind fruitful in suggestions of a profound and philosophic character—I mean that of Sir John Herschel—Mr. Barlow, of Woolwich, had experimented with a rotating iron shell. Mr. Christie had also performed an elaborate series of experiments on a rotating iron disk. Both of them had found that when in rotation the body exercised a peculiar action upon the magnetic needle, deflecting it in a manner which was not observed during quiescence; but neither of them was aware at the time of the agent which produced this extraordinary deflection. They ascribed it to some change in the magnetism of the iron shell and disk.

But Faraday at once saw that his induced currents must come into play here, and he immediately obtained them from an iron disk. With a hollow brass ball, moreover, he produced the effects obtained by Mr. Barlow. Iron was in no way necessary: the only condition of success was that the rotating body should be of a character to admit of the formation of currents in its substance: it must, in other words, be a conductor of electricity. The higher the conducting power, the more copious were the currents. He now passes from his little brass globe to the globe of the earth. He plays like a magician with the earth's magnetism. He sees the invisible lines along which its magnetic action is exerted, and sweeping his wand across these lines he evokes this new power. Placing a simple loop of wire round a magnetic needle he bends its upper portion to the west: the north pole of the needle immediately swerves to the east: he bends his loop to the east, and the north pole moves to the west. Suspending a common bar magnet in a vertical position he causes it to spin round its own axis. Its pole being connected with one end of a galvanometer wire, and its equator with the other end, electricity rushes round the galvanometer from the rotating magnet. He remarks upon the "*singular independence*" of the magnetism and the body of the magnet which carries it. The steel behaves as if it were isolated from its own magnetism.

And then his thoughts suddenly widen, and he asks himself whether the rotating earth does not generate induced currents as it turns round its axis from west to east. In his experiment with the twirling magnet the galvanometer wire remained at rest; one portion of the circuit was in motion *relatively* to *another portion*. But in the case of the twirling planet the galvanometer wire would necessarily be carried along with the earth; there would be no relative motion. What must be the consequence? Take the case of a telegraph wire with its two

terminal plates dipped into the earth, and suppose the wire to lie in the magnetic meridian. The ground underneath the wire is influenced like the wire itself by the earth's rotation; if a current from south to north be generated in the earth under the wire, a similar current from south to north would be generated in the earth under the wire; these currents would run against the same terminal plate, and thus neutralize each other.

This inference appears inevitable, but his profound vision perceived its possible invalidity. He saw that it was at least possible that the difference of conducting power between the earth and the wire might give one an advantage over the other, and that thus a residual or differential current might be obtained. He combined wires of different materials, and caused them to act in opposition to each other: but found the combination ineffectual. The more copious flow in the better conductor was exactly counterbalanced by the resistance of the worst. Still though experiment was thus emphatic he would clear his mind of all discomfort by operating on the earth itself. He went to the round lake near Kensington Palace, and stretched 480 feet of copper wire, north and south, over the lake, causing plates soldered to the wire at its ends to dip into the water. The copper wire was severed at the middle, and the severed ends connected with a galvanometer. No effect whatever was observed. But though quiescent water gave no effect, moving water might. He therefore worked at London Bridge for three days during the ebb and flow of the tide, but without any satisfactory result. Still he urges, "Theoretically it seems a necessary consequence, that where water is flowing there electric currents should be formed. If a line be imagined passing from Dover to Calais through the sea, and returning through the land, beneath the water, to Dover, it traces out a circuit of conducting matter one part of which, when the water moves up or down the channel, is cutting the magnetic curves of the earth, whilst the other is relatively at rest. . . . There is every reason to believe that currents do run in the general direction of the circuit described, either one way or the other, according as the passage of the waters is up or down the Channel." This was written before the submarine cable was thought of, and he once informed me that actual observation upon that cable had been found to be in accordance with his theoretic deduction.*

* I am indebted to a friend for the following exquisite morsel:—" A short time after the publication of Faraday's first researches in magneto-electricity, he attended the meeting of the British Association at Oxford, in 1832.—On this occasion he was requested by some of the authorities to repeat the celebrated experiment of eliciting a spark from a magnet, employing for this purpose the large magnet in the Ashmolean Museum. To this he consented, and a large party assembled to witness the

Three years subsequent to the publication of these researches, that is to say on the 29th of January, 1835, Faraday read before the Royal Society a paper "On the influence by induction of an electric current upon itself." A shock and spark of a peculiar character had been observed by a young man named William Jenkin, who must have been a youth of some scientific promise, but who, as Faraday once informed me, was dissuaded by his own father from having anything to do with science. The investigation of the fact noticed by Mr. Jenkin led Faraday to the discovery of the *extra current*, or the current *induced in the primary wire itself* at the moments of making and breaking contact, the phenomena of which he described and illustrated in the beautiful and exhaustive paper referred to.

Seven and thirty years have passed since the discovery of magneto-electricity; but, if we except the *extra current*, until quite recently nothing of moment was added to the subject. Faraday entertained the opinion that the discoverer of a great law or principle had a right to the "spoils"—this was his term—arising from its illustration; and guided by the principle he had discovered, his wonderful mind, aided by his wonderful ten fingers, overran in a single autumn this vast domain, and hardly left behind him the shred of a fact to be gathered by his successors.

And here the question may arise in some minds, What is the use of it all? The answer is, that if man's intellectual nature thirsts for knowledge, then knowledge is useful because it satisfies this thirst. If you demand practical ends, you must, I think, expand your definition of the term practical, and make it include all that elevates and enlightens the intellect, as well as all that ministers to the bodily health and comfort of men. Still, if needed, an answer of another kind might be given to the question, "What is its use?" As far as electricity has been applied for medical purposes it has been almost exclusively Faraday's electricity. You have noticed those lines of wire which cross the streets of London. It is Faraday's currents

experiments, which, I need not say, were perfectly successful. Whilst he was repeating them a dignitary of the university entered the room, and addressing himself to Professor Daniell, who was standing near Faraday, inquired what was going on. The Professor explained to him as popularly as possible this striking result of Faraday's great discovery. The Dean listened with attention and looked earnestly at the brilliant spark, but a moment after he assumed a serious countenance and shook his head; 'I am sorry for it,' said he as he walked away; in the middle of the room he stopped for a moment and repeated, 'I am sorry for it;' then walking toward the door, when the handle was in his hand he turned round and said, '*Indeed* I am sorry for it; it is putting new arms into the hands of the incendiary.' This occurred a short time after the papers had been filled with the doings of the hayrick burners. An erroneous statement of what fell from the Dean's mouth was printed at the time in one of the Oxford papers. He is there wrongly stated to have said, 'It is putting new arms into the hands of the infidel.'"

that speed from place to place through these wires. Approaching the point of Dungeness the mariner sees an unusually brilliant light, and from the noble *phares* of La Hève the same light flashes across the sea. These are Faraday's sparks exalted by suitable machinery to sunlike splendor. At the present moment the Board of Trade and the Bretheren of the Trinity House, as well as the Commissioners of Northern Lights, are contemplating the introduction of the magneto-electric light at numerous points upon our coasts; and future generations will be able to refer to those guiding stars in answer to the question, What has been the practical use of the labors of Faraday? But I would again emphatically say that his work needs no such justification, and that if he had allowed his vision to be disturbed by considerations regarding the practical use of his discoveries, those discoveries would never have been made by him. "I have rather," he writes in 1831, "been desirous of discovering new facts and new relations dependent on magneto-electric induction, than of exalting the force of those already obtained; being assured that the latter would find their full development hereafter."

In 1817, when lecturing before a private society in London on the element chlorine, Faraday thus expresses himself with reference to this question of utility:—"Before leaving this subject, I will point out the history of this substance, as an answer to those who are in the habit of saying to every new fact, 'What is its use?' Dr. Franklin says to such, 'What is the use of an infant?' The answer of the experimentalist is, 'Endeavor to make it useful.' When Scheele discovered this substance it appeared to have no use; it was in its infancy and useless state, but having grown up to maturity, witness its powers, and see what endeavors to make it useful have done."

Art. VII.—*Chemical Apparatus;* by W. P. Dexter.

Gas lamps for the ignition of crucibles, &c.—The ordinary Bunsen burner is known to act upon the surface of platinum vessels brought in contact with the inner line of the flame: the metal loses its polish, becoming superficially porous and spongy, and requires the use of the burnisher to bring it back to its original state. This alteration of the surface I have found to be attended with a change of weight, so that for some years I have used a lamp of different construction for the heating of platinum crucibles in analytical operations. Such a lamp may be made by removing the air tube of a common Bunsen lamp and putting in its place a somewhat

longer one of glass or iron of about 12 millimeters internal diameter. The gas jet should have a single circular aperture, and be in proper proportion to the diameter of the tube, which may be held in any of the ordinary clamp supports. The tube being raised sufficiently above the jet to allow free entrance of air, and a full stream of gas let on, a "roaring" flame is produced, of which the interior blue cone is pointed, sharply defined, and extends only about half an inch from the top of the tube. A polished platinum surface is not acted upon by this flame provided it be not brought into contact with the interior cone. In the Bunsen burner, as usually made, the supply of air depends upon the diameter of the tube, the holes at its base being more than sufficient to supply the draught. With the wider tube it is necessary to limit the admission of air by depressing the tube upon the lamp when the force of the gas is diminished. Otherwise the proportion becomes such that an explosive mixture is formed; for this reason it is more convenient to use an arrangement in which the access of air can be regulated by an exterior tube sliding obliquely downward over the air apertures. The gas jet should be on a level with the top of these apertures, which must be much larger than those of the ordinary Bunsen's burner.

On account of the liability to explode and burn at the jet inside, the lamp is not well adapted for ordinary use; but for ignition of crucibles, working of glass, &c., it has proved efficient and practical.

Gas regulator.—Now that gas is universally used as a source of heat in laboratories, it is desirable to have a means of keeping the pressure constant, and independent of the changes which take place in the mains. By the Regulator of Kemp a uniform temperature can be kept up for any length of time; but this apparatus is a little difficult to adjust, is not universally applicable, and can be used in but one operation at a time. By regulating the pressure of the gas, on the other hand, a constant supply of heat is furnished, but the temperature is not so exactly maintained in consequence of the more or less rapid abstraction of heat by change of temperature of the locality, and from currents of air. It is, however, as constant as it can be kept by a spirit lamp, which we are at present often obliged, from the variations of pressure, to substitute for gas; while the regulator may be connected with several burners, or even include the whole laboratory, if the gas pipes are of sufficient size.

The arrangement which I have had in use for the last year consists of a common gasometer, made of zinc, and about 9 inches in height and diameter. The floating bell is connected

by a jointed rod with a stopcock in the pipe by which the gas enters. When the bell rises from the pressure of the gas, it gradually closes this cock, and thus cuts off the further supply. The gas is then under a constant pressure depending upon the weight of the bell; as gas is consumed the bell sinks and opening the cock allows more to flow in. The difference of weight of the bell from its greater or less immersion in the water is inappreciable; a very slight diminution of pressure, hardly perceptible without a microscope, is observed when one of the outlets is suddenly thrown open to its full extent, and is due, probably, to the friction of the gas in the tubes, which should therefore be of considerable size.

The apparatus should not be painted, as oil is acted upon by water which has been long in contact with gas. Asphaltum varnish seems to answer better.

Art. VIII.—*On the Equivalent of Cerium;* by the late Dr. Charles Wolf, of Cincinnati, Ohio.

At the suggestion of Professor Bunsen in Heidelberg, the father of the late Dr. Charles Wolf of Cincinnati, Ohio, placed in my hands, papers and preparations relating to an investigation, which his son had made, while in the laboratory of Prof. Bunsen, and requested me to collate the same and prepare them for publication. This task I cheerfully accepted, the more, that I deemed the death of this young and promising chemist a real loss to the cause of science.

I here give a translation of Dr. Wolf's investigation on the equivalent of cerium, which Bunsen pronounces very valuable. I have examined all the data with the greatest care and gone over a great many of his analyses and calculations. The paper was about complete, when it came into my hands, all but a few of the last pages, which were in the shape of notes, but these were sufficiently copious to enable me to finish it, with the aid of some analyses, which I found among his papers.

I could not find the description of the crystalline form of his salts, to which he refers, nor is there the least evidence that any measurements have ever been made. This investigation may be of value to chemists laboring in the same field, as it designates the probable existence of *another* foreign substance in the "cerite-oxyds," and we must deeply regret that its author was not spared long enough to conclude his researches on the nature of this oxyd, which so considerably increases the equivalent of cerium.

Let us hope that the hints and suggestions thrown out by

Dr. Wolf may not be lost, but that they may induce some investigator to advance or conclude this highly interesting subject. F. A. GENTH.

Philadelphia, April 26th, 1868.

Since the discovery of the peculiar oxyds contained in the cerite by Berzelius & Hisinger* and Mosander† they have been the subject of numerous researches. Their occurrence together with their very similar chemical properties presented very great difficulties in their separation and indeed the mixture of the three oxyds was for a long time looked upon as the oxyd of *one metal only*, which was called *cerium*. Notwithstanding the many investigations made on this subject, the methods for the separation and purification of the three cerite-oxyds are very imperfect.

The oxyd of cerium, which in its properties differs most from the oxyd of didymium and from lanthana, and which is the most common, is more readily obtained in a state of perfect purity than either of the two others.

The following investigation was made for the purpose of preparing salts of cerium of *absolute* purity, so as to determine from them the equivalent of the metal. The material used was Swedish cerite.

The method adopted for the preparation of the oxyds was substantially that recommended by Bunsen,‡ with some slight alterations, as will be seen from the following description.

The finely pulverized mineral, mixed in a porcelain dish with oil of vitriol to the consistency of a stiff paste, was put into a hessian crucible and exposed to a dull red heat. The excess of sulphuric acid was driven off and the ash-grey mass finely pulverized and gradually added to a large dish full of ice-water and ice. It was then agitated until the water had become saturated, when it was allowed to settle. The saturated solution was afterward poured off and the residue washed until the liquid became tasteless and gave no further precipitate with oxalic acid.

After having been concentrated by evaporation this solution was precipitated by sulphydric acid and filtered. The excess of sulphydric acid was driven off by heat, the liquid was then oxydized by chlorine, after which the excess of the latter was also expelled by heat. It was then strongly acidulated by chlorhydric acid, heated up to boiling and precipitated by a boiling solution of oxalic acid, while being constantly stirred. The precipitate of crystalline oxalates, which settled easily, was

* Berzelius & Hisinger, Gehlen's Journ. der Chemie, ii, 397.

† Mosander, Journ. fur pract. Chemie, xxx, 1843; Pogg. Ann., clvi, 1843; Edin. & Dubl. Phil. Mag., Oct. 1843.

‡ Bunsen, Annalen der Chem. & Pharm., cv, 1858.

decanted and washed with boiling water. The mother-liquor still contained a large portion of the rare oxyds, but these were very much mixed with other bases.

The dried oxalates were then placed into a porcelain dish and decomposed by heat over an open fire, care being taken to stir them constantly. Should this precaution be neglected they might cake together, in which case a portion of the salt would remain undecomposed. Concentrated nitric acid was now poured upon the cinnamon-brown powder and the mixture was heated until the oxyds were dissolved.

The deep red violet solution was freed as much as possible from the excess of nitric acid by evaporation, and while still warm it was poured in a boiling mixture of water and sulphuric acid, containing 2 c. c. of sulphuric acid per liter of water.

The red violet color at once disappeared, and while a great evolution of gas took place, a dirty yellow precipitate of basic ceroso-ceric sulphate was formed. The mixture was allowed to boil up two or three times; it was then removed from the fire, when the precipitate settled rapidly.

Should the quantity of sulphuric acid and water have been insufficient, a portion of the nitric acid solution remains undecomposed and the supernatant liquid is yellow; this is also the case when the sulphuric acid water is too concentrated, because then a portion of the precipitate is dissolved by the excess of the acid. If the right proportions have been used, the supernatant liquid will have an amethystine color from the presence of didymium; in it neither boiling nor the addition of water or a few drops of sulphuric acid will give a precipitate. A few preliminary experiments will determine the requisite quantities of water and sulphuric acid.

The basic precipitate was washed by decantation, at first with boiling water containing per liter 1 c. c. of sulphuric acid, then with boiling distilled water. The precipitate settles easily. It is well to preserve the first wash-waters, since a portion of the precipitate dissolves in them even to the extent of one third. The last portion of the filtrate resulting from the washings with pure water need not be saved, as they contain traces only of the precipitate.

This basic sulphate, although sometimes prepared by a different process, has been used by most chemists as a starting point for the preparation of pure salts of cerium, and the sulphate obtained from it has served for the determination of its equivalent.

The washed basic salt, obtained by the method above mentioned, I divided into two equal parts; with the one I repeated the experiments made by previous observers, while the other

supplied me with the material for some new experiments, which I shall detail in due season. The first part was dissolved by the addition of a few drops of sulphuric acid and slightly heated, the yellowish-red solution reduced by sulphurous acid and evaporated to dryness. The dry salt, heated in a platinum crucible, was freed from its excess of sulphuric acid and water; the cerous sulphate therefrom resulting was dissolved in cold water, filtered and very slowly evaporated over a water bath. The slower the evaporation the better will the crystals be.

When but little of the mother-liquor remained, this was poured off, the salt washed twice with boiling water, and the purified salt dried over a water bath.

Upon being tested with the spectroscope, the mother-liquor showed the whole absorption spectrum of didymium, with the same distinctness as a moderately concentrated solution of didymium would do.

A solution of the purified crystals gave such lines as in the didymium spectrum are usually marked by their intensity. A small portion of the mother-liquor precipitated by oxalic acid gave on ignition of the oxalate an oxyd of a brown color, while the ceroso-ceric oxyd obtained by a similar process from the sulphate had already a brick-red color. For the purpose of further purification the whole mass of the sulphate was recrystallized five times in the manner above stated and the mother-liquor always separated from the crystals, then the salt was separated by fractional crystallization into five equal portions, as follows:

The mass of the crystals	into	A_1 ; A_2 ; A_3
A_1	"	B_1 ; B_2
B_1	"	C_1 ; C_2

Of these salts B_2, C_1 and C_2 were analyzed.

The following method was in general adopted: About 1·5 grams of the salt, which had previously been dried over chlorid of calcium, was weighed in a tared platinum crucible; this was transferred into a larger platinum crucible, in the bottom of which was a thick platinum spire, upon which the smaller crucible rested, fully surrounded by air. The large crucible covered by a mica plate was then very carefully heated during three hours, high over a small gas flame, and weighed. These weighings were repeated, the crucible remaining at the same height over the flame, while the heat was gradually increased, until finally a point was reached, when, notwithstanding a considerable increase in the temperature, the weight of the crucible remained invariable. From five to eight weighings generally sufficed to give the amount of water.

The anhydrous salt was then dissolved in a large quantity of water, the solution heated and precipitated by a concentrated boiling solution of oxalic acid. The filtrate should always be tested by ammonia and should not give a precipitate, if the proportions of oxalic acid are correct.

The greatest precautions are necessary in the ignition of the oxalate, because the resulting ceroso-ceric oxyd is in such an exceedingly finely divided condition that the slightest shaking will occasion a loss, unless the crucible be covered by a well fitting lid.

The resulting ceroso-ceric oxyd was always analyzed and the amount of cerous oxyd corresponding with it determined according to Bunsen's volumetric method by iodid of potassium and chlorhydric acid from the amount of liberated iodine.

1.—1·4542 grs. of B_2 gave 0·19419 grs. water and 0·76305 grs. ceroso-ceric oxyd, corresponding with 0·72443 grs. cerous oxyd; 0·70325 grs. of ceroso-ceric oxyd gave 0·66766 grs. of cerous oxyd.

2.—1·4104 grs. of C_1 gave 0·1898 grs. water and 0·7377 grs. of ceroso-ceric oxyd, giving 0·70217 grs. of cerous oxyd.

3.—1·35027 grs. of C_2 gave 0·1820 grs. water and 0·70665 grs. of ceroso-ceric oxyd, corresponding with 0·67261 grs. cerous oxyd; 0·6916 grs. ceroso-ceric oxyd gave 0·65829 grs. cerous oxyd.

According to these results the anhydrous sulphate contains as follows:

	B_2	C_1	C_2	Mean.
Cerous oxyd,	57·494	57·526	57·574	57·531
Sulphuric acid,	42·506	42·474	42·426	42·469
	100·000	100·000	100·000	100·000

B_2 would give for the equivalent of cerium		46·104
C_1	" " "	46·176
C_2	" " "	46·281

giving as the mean result=46·187.

The anhydrous sulphate consists according to these numbers of one equivalent of base for one of acid.

The composition of the crystals is as follows:

	B_2	C_1	C_2	Mean.	Calculated.
Cerous oxyd,	49·816	49·785	49·813	49·805	49·628
Sulphuric acid,	36·830	36·758	36·708	36·765	36·634
Water,	13·354	13·457	13·479	13·430	13·738
	100·000	100·000	100·000	100·000	100·000

These results agree with the calculated analysis, which corresponds to the formula: $3(CeO, SO_3)+5HO$.

The equivalent 46·187 has been used for these calculations. The salt, which gave these results was crystallized in small crystals, elongated in one direction, the crystallographic description of which I shall subsequently give.

Let us compare this equivalent with the results which were previously obtained by other observers.

Beringer* made in Wöhler's laboratory four determinations of the equivalent of cerium. His salts were all prepared from the residue, remaining after the extraction of the mixed cerite-oxyds by dilute nitric acid. They were all rose-colored.

From the pale rose-colored cerous sulphate he obtained the number:	577·24	46·179
From the like colored cerous chlorid,	576·97	46·158
By the conversion of the sulphid into oxyd,	576·69	46·127
From the cerous formate,	576·00	46·080
Mean,	576·73	46·138

Hermann† found the number 575·00=46·00 by an analysis of the cerous sulphate prepared from the basic sulphate. In this case likewise the mixed oxyds were first extracted by nitric acid.

Marignac‡ used for his determinations the cerous sulphate, which was prepared from the basic sulphate, purified by several re-crystallizations. The basic sulphate was prepared from the mixed oxyds, remaining after having been extracted by nitric acid. The residue was dissolved in sulphuric acid, and the basic salt precipitated by dilution with water. Marignac adopts the number 46·00 as the equivalent of cerium.

Under the guidance of Bunsen,§ Jegel made several experiments. The mixed oxalates were ignited with *magnesia alba*, then dissolved in boiling nitric acid and the solution evaporated to crystallization. The solution of these crystals in water, when poured in boiling water, containing sulphuric acid, produced the basic ceric sulphate, from which the neutral cerous salt was obtained, which gave:

$$576{\cdot}3 \quad 575{\cdot}25 \quad 575{\cdot}65 \qquad \text{Mean}\begin{cases}575{\cdot}73\end{cases}$$

$$46{\cdot}104 \quad 46{\cdot}020 \quad 46{\cdot}050 \qquad \phantom{\text{Mean}} \;\; 46{\cdot}058$$

Rammelsberg|| obtained by the analysis of the cerous oxalate the number 575·90=46·072.

* Beringer, Annalen der Chemie & Pharm., xlii, 1842.

† Hermann, Journ. für Pract. Chemie, xxx, 1843.

‡ Marignac, Ann. de Chimie et Phys., III, xxxviii, 148; Ann. der Chem. und Pharm., lxviii, 1848.

§ Bunsen, Annalen der Chem. & Pharm., cv, 1858.

|| Rammelsberg, Poggendorf's Ann., cviii, 1859.

To facilitate comparison I will tabulate the different results:

	O=100	H=1
Beringer,	576·73	46·138
Hermann,	575·00	46·000
Marignac,	575·00	46·000
Bunsen,	575·73	46·058
Rammelsberg,	575·90	46·072
Wolf,	577·33	46·187

The new number differs from all the others and is even higher than that obtained by Beringer with far less purified material; all his salts were rose-colored, owing to the presence of didymium, my salts, on the contrary, were perfectly colorless, each had been re-crystallized at least ten times (always rejecting the mother-liquor).

Having used the very greatest care both in the preparation and in the analyses of the salt, it is difficult to account for this high result; either the same analytical error has been committed in the three analyses, or there may be present *another* foreign substance besides didymium. The first supposition is contradicted by analyses, which have been made by exactly the same methods and which will be given hereafter, while the second I shall discuss presently.

A sample of salt, which had been re-crystallized twenty times, still distinctly showed the line D of the absorption spectrum of didymium, and it being evident that by crystallization *alone* no absolutely pure cerous sulphate can be prepared, it was therefore necessary to search for another method for its preparation. To ascertain the purity of the resulting product the cerous oxalate was used, and from the color of the oxyd produced by its ignition the state of purity was readily ascertained.

Mosander had already correctly stated that the brown color of the mixed oxyds was due to the presence of foreign substances; he obtained the ceroso-ceric oxyd of a reddish white color; Hermann had it of an isabel color. By a slight alteration in the mode of preparation, Bunsen obtained it of a yellowish white color. On my part I have made numerous experiments in order to produce it as white as possible.

For this purpose the previously mentioned second portion of the basic sulphate was used. Out of the numerous methods which I have tried, but *two* have given encouraging results.

The first was as follows: A small portion of the precipitate was dissolved by heating it with a few drops of sulphuric acid and this solution precipitated by pouring it into a large quantity of boiling water. After its conversion into oxalate the new precipitate gave a paler ceroso-ceric oxyd; a repetition of

the same process still gave a better result, but the reduction in color was not very rapid and it would have required at least eight or ten precipitations to produce as pale an oxyd as that obtained by the following process; besides, another disadvantage, incident to these numerous washings, is the very large quantity of solution and the great loss of material, which are the consequences. Far better results were obtained by the use of nitric instead of sulphuric acid. The crude precipitate was dissolved in a small quantity of nitric acid, and the deep red solution treated as above, by which a far paler precipitate of basic salt was obtained. The resulting mother-liquor gave on evaporation the complete didymium spectrum and the color of the oxyds prepared from it was brown like that of the original mixed oxyds.

To save repetitions, I will call the crude basic salt N and the precipitate resulting from it Nα. Nα was re-dissolved again in nitric acid and the solution treated as above; it gave a new Nβ, which was easily discernible from N and Nα by its paler color and greater purity. It separated far more readily from the supernatant liquid than N and Nα and was more easily washed out.

The mother-liquor from Nβ examined with the spectroscope still indicated the presence of didymium, and Nβ itself yielded a yellowish white ceroso-ceric oxyd, in which *traces* of didymium were still perceptible.

A portion of Nβ was converted into sulphate and this was re-crystallized six times, the mother-liquor being always separated at each operation. The crystallizations were obtained by a very slow evaporation of the liquid, and I observed that the forms of the crystals had materially changed. As already stated, the still impure sulphate always gave thin slender crystals, while now, after repeated evaporations and crystallizations, no other but small octahedral forms were produced.

From this salt several new determinations of the equivalent were made.

The pulverized salt was dried over sulphuric acid and the analyses conducted as already described:

β_1. 1·4327 grs. gave 0·2733 grs. water and 0·69925 grs. of ceroso-ceric oxyd, corresponding with 0·66491 grs. cerous oxyd;

β_2. 1·5056 grs. gave 0·2775 grs. water and 0·7405 grs of ceroso-ceric oxyd, corresponding with 0·70413 grs. of cerous oxyd; 0·8816 grs. of the ceroso-ceric oxyd from β_1 and β_2 gave 0·8383 grs. cerous oxyd.

β_3. 1·44045 grs. gave 0·2710 grs. water and 0·7052 grs. ceroso-ceric oxyd, corresponding with 0·67044 grs. cerous oxyd. 0·5102 grs. of the ceroso-ceric oxyd gave 0·48505 grs. cerous oxyd.

These results give for the anhydrous sulphate:

	β_1	β_2	β_3	Mean.
Cerous oxyd,	57·349	57·335	57·329	57·338
Sulphuric acid,	42·651	42·665	42·671	42·662
	100·000	100·000	100·000	100·000
Equivalent,	45·784	45·754	45·741	45·760

The hydrous sulphate has the composition:

	β_1	β_2	β_3	Mean.
Cerous oxyd,	46·409	46·767	46·544	46·573
Sulphuric acid,	34·515	34·802	34·642	34·653
Water,	19·076	18·431	18·814	18·774
	100·000	100·000	100·000	100·000

From these analyses it becomes evident that the composition of this hydrous sulphate differs from that previously described and that it can be expressed by the formula $2(CeO, SO_3)+5HO$, which would give:

Cerous oxyd,	46·241
Sulphuric acid,	34·406
Water,	19·353

The equivalents resulting from these analyses differ very much from those previously obtained, but it is hardly possible that the separation of didymium *alone* can be the cause of the reduction from 46·187 to 45·760. Bunsen's and Rammelsberg's equivalents 46·058 and 46·072 were certainly made with cerium salts *free* or *almost free* from didymium. The salt which gave 45·760 still contained a trace of this metal.

From all these facts we are led to conclude that the reduction of the equivalent of cerium from 46· to 45·760 is *not* owing to the separation of didymium, but to that of *another foreign* substance.

The mother-liquor and wash-waters from Nβ gave with ammonia a very minute precipitate.

Encouraged by the results obtained by this method of separation, I have continued in the same manner. A portion of Nβ was dissolved in nitric acid, and this solution re-precipitated by boiling water. The new precipitate Nγ was *almost* white. In the mother-liquor the presence of didymium could hardly be detected by the line D, while in the precipitate itself *not a trace* was visible.

The sulphate prepared from Nγ again showed the habitus of the first sulphates and crystallized in slender crystals, which gave the same angles with no new modifications; analysis proved that their composition also was nearly the same. I found that—

1·4684 grs. gave 0·1880 grs. of water and 0·7717 grs. of ceroso-ceric oxyd, which gave 0·7338 grs. of ceroso-oxyd.

The sulphate contains therefore as follows:

	The anhydrous:	The hydrous:
Cerous oxyd,	57·310	49·973
Sulphuric acid,	42·690	37·224
Water,	-----	12·803
	100·000	100·000

The equivalent calculated from this analysis is again lower, being equal to 45·699.

Although the cerous sulphate from which the last determinations had been made was *entirely free* from didymium, still I thought that a repetition of the same mode of purification might lead to a still greater reduction of the equivalent, owing to the removal of the foreign substance above alluded to.

A portion of Nγ was therefore treated as usual, and produced the basic salt Nδ.

After washing it was perfectly white, though otherwise in appearance similar to N, Nα, Nβ and Nγ.

The oxyds obtained, both from the oxalate, prepared from the mother-liquor, and that from the basic salt, were white. In neither could the least trace of didymium be detected. The sulphate prepared from Nδ after repeated and very careful recrystallizations, had the habitus of the first sulphates and gave on analysis the following results:

1·3756 grs. gave 0·1832 grs. water and 0·7186 grs. of ceroso-ceric oxyd, yielding 0·68318 grs. of cerous oxyd.

These results would give for:

	The crystallized salt.	The anhydrous salt.
Cerous oxyd,	49·664	57·294
Sulphuric acid,	37·018	42·706
Water,	13·318	-----
	100·000	100·000

The equivalent was again reduced by this operation, which now makes it equal to 45·664, which number must be considered as the equivalent of the purest cerium, which has as yet been prepared.

Further investigations will be needed to ascertain, whether a continued repetition of the same operation can reduce the equivalent of cerium to a still lower number.

ART. IX.—*Laws of Botanical Nomenclature adopted by the International Botanical Congress held at Paris in August,* 1867; *together with an Historical Introduction and Commentary.* By ALPHONSE DECANDOLLE. Translated from the French.

*** We reprint, from the English Translation, published by L. Reeve & Co., (London, 1868, pp. 72, 8vo) the

LAWS OF BOTANICAL NOMENCLATURE ADOPTED BY THE CONGRESS.

General considerations and leading principles.

ARTICLE 1. Natural History can make no real progress without a regular system of nomenclature, acknowledged and used by a large majority of naturalists of all countries.

Art. 2. The rules of nomenclature should neither be arbitrary, nor imposed by authority. They must be founded on considerations clear and forcible enough for every one to comprehend and be disposed to accept.

Art. 3. The essential point in nomenclature is to avoid or to reject the use of forms, or names, that may create error or ambiguity, or throw confusion into science.

Next in importance is the avoidance of any useless introduction of new names.

Other considerations, such as absolute grammatical correctness, regularity or euphony of names, a more or less prevailing custom, respect for persons, etc., notwithstanding their undeniable importance, are relatively accessory.

Art. 4. No custom contrary to rule can be maintained if it leads to confusion or error. When a custom offers no serious inconvenience of this kind, it may be a motive for exceptions, which we must, however, abstain from extending or imitating. In the absence of rule, or where the consequences of rules are questionable, established custom becomes law.

Art. 5. The principles and forms of nomenclature should be as similar as possible in botany and in zoology.

Art. 6. Scientific names should be in Latin. When taken from another language, a Latin termination is given to them, except in cases sanctioned by custom. If translated into a modern language, it is desirable that they should preserve as great a resemblance as possible to the original Latin names.

Art. 7. Nomenclature comprises two categories of names :—1. Names, or rather terms, expressing the nature of the groups comprehended one within another. 2. Names particular to each of the groups of plants or animals that observation has made known to us.

On the manner of designating the nature and subordination of the groups that constitute the Vegetable Kingdom.

Art. 8. Every individual plant belongs to a species (*species*), every species to a genus (*genus*), every genus to an order (*ordo, familia*), every order to a cohort (*cohors*), every cohort to a class (*classis*), every class to a division (*divisio*).

Art. 9. In many species we distinguish likewise *varieties* and *variations*, and in some cultivated species, modifications still more numerous; in many genera *sections*, in many orders *tribes*.

Art. 10. Finally, if circumstances require us to distinguish a greater number of intermediate groups, it is easy, by putting the syllable *sub* before the name of the group, to form subdivisions of that group; in this manner suborder (*subordo*) designates a group between an order and a tribe, subtribe (*subtribus*), a group between a tribe and a genus, etc. The *ensemble* of subordinate groups may thus be carried, for uncultivated or spontaneous plants only, to twenty degrees, in the following order:—

Regnum vegetabile.
Divisio.
Subdivisio.
Classis.
Subclassis.
Cohors.
Subcohors.
Ordo.
Subordo.
Tribus.
Subtribus.
Genus.
Subgenus.
Sectio.
Subsectio.
Species.
Subspecies (vel Proles, Angl. *Race*).
Varietas.
Subvarietas.
Variatio.
Subvariatio.
Planta.

Art. 11. The definition of each of these names of groups may vary, in a certain degree, according to individual opinion and the state of science, but their relative rank, sanctioned by custom, must not be inverted. Any classification containing inversions, such as the division of genera into orders, or of species into genera, is inadmissible.

Art. 12. The fertilization of one species by another gives rise to a hybrid (*hybridus*); that of a modification or subdivision of a species by another modification of the same species produces a half-breed (*mistus*, *mule* of florists).

Art. 13. The arrangement of species in a genus or in a sub-

division is made by means of typographical signs, letters, or figures. Hybrids are classed after one of the species from which they originate, with the sign × prefixed to the generic name.

The rank of subspecies under species is marked by letters or figures; that of varieties by the series of Greek letters α, β, γ, etc. Groups below varieties and half-breeds (mule of florists) are indicated by letters, figures or typographical signs, according to the will of the author.

Art. 14. Modifications of cultivated species should, where possible, be classed under the wild or spontaneous species from which they are derived.

For this purpose the most striking are treated as subspecies, and when constant from seed, they are called races (*proles*).

Modifications of a secondary order take the name of varieties, and if there be no doubt as to their almost constant heredity by seed, they are termed subraces (*subproles*).

Modifications of minor importance, more or less comparable to subvarieties, variations or subvariations of uncultivated species, are indicated according to their origin in the following manner:—1. *Satus* (seedling; Gall. semis; Germ. Sämling), for a form obtained from seed. 2. *Mistus* (blending;* Gall. métis; Germ. Blendling), for a form arising from cross-fertilization in a species. 3. *Lusus* (sport; Germ. Spielart), for a form originating from a leaf-bud or from any other organ, and propagated by division.

On the manner of designating each group or association of plants.

Section 1. *General Principles.*

Art. 15. Each natural group of plants can bear in science but one valid designation, namely, the most ancient, whether adopted or given by Linnæus, or since Linnæus, provided it be consistent with the essential rules of nomenclature.

Art. 16. No one ought to change a name or a combination of names without serious motives, derived from a more profound

* Since the meeting of the Congress, the author of this pamphlet has, together with the translator, turned his attention to the choice of a significant English term for the French *métis*. The word *blending* does not perhaps indicate quite clearly enough the existence of a mixture, and does not allude to its nature. The term *half-breed*, used by agriculturists, appears to answer much better to the sense *métis*; *breed* precisely implying a *race*, and *half-breed* the mixture of two races. It may, however, likewise be suggested that the shortness of the French word *métis*, analogous to the Spanish *mestizo*, and evidently derived from the Latin *mistus*, or *mixtus*, will perhaps induce English botanists to adopt it, together with the word half-breed. The latter is undoubtedly more expressive, but metis has over it the advantage of being intelligible in several tongues. The term *mule*, as applied to the mixture of varieties of races, is in constant use amongst English florists; but is too obviously erroneous to be sanctioned by scientific writers. (*Translator.*)

knowledge of facts, or from the necessity of relinquishing a nomenclature that is in opposition to essential rules (art. 3, first paragraph, 4, 11, 15, etc.: see sect. 6).

Art. 17. The form, the number, and the arrangement of names depend upon the nature of each group, according to the following rules.

Section 2. *Nomenclature of the different kinds of Groups.*

§ 1. *Names of Divisions and Subdivisions, Classes and Subclasses.*

Art. 18. The names of divisions and subdivisions, of classes and subclasses, are drawn from their principal characters. They are expressed by words of Greek or Latin origin, some similarity of form and termination being given to those that designate groups of the same nature (Phanerogams, Cryptogams; Monocotyledons, Dicotyledons, etc.).

Art. 19. Among Cryptogams, the old family names, such as *Filices*, *Musci*, *Fungi*, *Lichenes*, *Algæ*, may be used for names of classes and subclasses.

§ 2. *Names of Cohorts and Subcohorts.*

Art. 20. Cohorts are designated preferably by the name of one of their principal Orders, and as far as possible with a uniform termination.

Subcohorts (rarely used) may be designated in the same manner.

§ 3. *Names of Orders and Suborders, of Tribes and Subtribes.*

Art. 21. Orders (*Ordines*, *Familiæ*) are designated by the name of one of their genera, with the final *aceæ* (*Rosaceæ*, from *Rosa*; *Ranunculaceæ*, from *Ranunculus*, etc.).

Art. 22. Custom warrants the following exceptions:—

(1.) When the Latin name of the genus from which is taken that of the Order ends in *-ix* or *-is* (genitive *-icis* or *-idis*), the termination *-iceæ*, or *-ineæ*, or *-ideæ* is admitted (*Salicineæ*, from *Salix; Tamariscineæ*, from *Tamarix; Berberideæ*, from *Berberis*).

(2.) When the genus from which the name is derived has an unusually long name, no tribe in the Order taking its appellation after the same genus, the termination in *-eæ* is admitted (*Dipterocarpeæ*, from *Dipterocarpus*).

(3.) Some large Orders, named long since, have retained the exceptional names under which they are generally known (*Cruciferæ*, *Leguminosæ*, *Guttiferæ*, *Umbelliferæ*, *Compositæ*, *Labiatæ*, *Cupuliferæ*, *Coniferæ*, *Palmeæ*, *Gramineæ*, etc.).

(4.) An old generic name, which has become that of a section or of a species, may be preserved as the foundation of that of the Order (*Lentibularieæ*, from *Lentibularia; Hippocastaneæ*,

from *Æsculus Hippocastanum; Caryophylleæ*, from *Dianthus Caryophyllus*, etc.).

Art. 23. The names of suborders (*subordines, subfamiliæ*) are derived from the name of one of the genera that form part of them, with the final *-eæ*.

Art. 24. The names of tribes and subtribes are taken from that of one of the genera included in the group, with the final *-eæ* or *-ineæ*.

§ 4. *Names of Genera and of Divisions of Genera.*

Art. 25. Genera, subgenera, and sections, receive names, commonly substantive, which may be compared to our own proper family names.

These names may be derived from any source whatsoever, and may even be arbitrarily imposed, under the restrictions mentioned further on.

Art. 26. A name may be given to subsections, as well as to inferior generic subdivisions; or these may simply be indicated by a number, or by a letter.

Art. 27. When the name of a genus, subgenus, or section is taken from the name of a person, it is composed in the following manner:—

The name cleared of titles or of any accessory particle, takes the final *-a* or *-ia*.

The spelling of the syllables unaffected by this final, is preserved without alteration, even with letters or diphthongs now employed in certain languages, but not in Latin. Nevertheless *ä, ö*, and *ü*, of the German language become *æ, œ*, and *ue*, whilst *é* and *è* of the French language become *e*.

Art. 28. Botanists who have generic names to publish show judgment and taste by attending to the following recommendations:—

(1.) Not to make names too long or difficult to pronounce.

(2.) To give the etymology of each name.

(3.) If they have formerly made a name that has not been accepted, not to establish another genus under the same name, particularly in the same Order, or in a neighboring one.

(4.) Not to dedicate genera to persons in all respects strangers to botany, or at least to natural history, nor to persons quite unknown.

(5.) Not to draw names from barbarous tongues, unless those names be frequently quoted in books of travel, and have an agreeable form that adapts itself readily to the Latin tongue, and to the tongues of civilized countries.

(6.) If possible, by the composition or the termination of the word, to call to mind the affinities or the analogies of the genus.

(7.) To avoid adjective nouns.

(8.) Not to give to a genus a name whose form is more properly that of a section (*Eusideroxylon*, for example).

(9.) To avoid taking up names that have already been used, but have not been approved, and applying them to genera different from the former, unless it be wished again to dedicate a genus to a botanist; but, even in this case, it is desirable—1, that the nullity of the first genus should be unquestionable; 2, that the order in which it is proposed to re-establish the name be quite distinct from the former one.

(10.) To avoid making choice of names used in zoology.

Art. 29. Botanists constructing names for subgenera or for sections will do well to attend to the recommendations of the foregoing article, as well as to these:—

(1.) Give, where possible, to the principal division of a genus a name that, by some modification or addition, may call the name of the genus to mind (for instance, *eu* at the beginning of the name, when it is of Greek origin; *-astrum*, *-ella*, at the end of a name, when Latin, or any other modification consistent with the rules of grammar and the usages of the Latin language).

(2.) Avoid calling a section by the name of the genus it belongs to, with the final *-oides* or *-opsis;* give, on the contrary, the preference to this final for a section having some resemblance to another genus, by adding, in that case, *-oides* or *-opsis* to the name of that other genus, if it be of Greek derivation, so as to form the name of the section.

(3.) Avoid taking, as a sectional name, one already in use as such, in another genus, or which is that of a genus.

Art. 30. When it is required to express the name of a section, together with a generic name and that of a species, the name of the section is put between the two others in a parenthesis.

§ 5. *Names of Species, of Hybrids, and of Subdivisions of Species, either spontaneous or cultivated.*

Art. 31. All species, even those that singly constitute a genus, are designated by the name of the genus to which they belong, followed by a name termed specific, more commonly of the adjective kind.

Art. 32. The specific name ought, in general, to indicate something of the appearance, the characters, the origin, the history, or the properties of the species. If derived from the name of a person, it usually calls to mind the name of him who discovered or described it, or who may have been otherwise concerned with it.

Art. 33. Names of persons used as specific names have a genitive or an adjective form (*Clusii* or *Clusiana*). The first is used when the species has been described or distinguished by the botanist whose name it takes; in other cases the second form is preferred. Whatever be the form chosen, every specific name derived from the name of a person should begin with a capital letter.

Art. 34. A specific name may be an old generic name, or a substantive proper name. It then takes a capital, and does not agree with the generic name (*Digitalis Septrum*, *Coronilla Emerus*).

Art. 35. No two species of the same genus can bear the same specific name, but the same specific name may be given in several genera.

Art. 36. In constructing specific names, botanists will do well to give attention to the following recommendations:—

(1.) Avoid very long names, as well as those that are difficult to articulate.

(2.) Avoid names that express a character common to all, or to almost all the species of a genus.

(3.) Avoid names designating little known or very limited localities, unless the species be very local.

(4.) Avoid, in the same genus, names too similar in form,—above all, those that only differ in their last letters.

(5.) Readily adopt unpublished names found in travellers' notes or in herbaria, unless they be more or less defective (see Art. 17).

(6.) Avoid names that have been already used in the genus, or in some nearly allied genus, and have become synonyms.

(7.) Name no species after any one who has neither discovered, nor described, nor figured, nor studied it in any way.

(8.) Avoid specific names composed of two words.

(9.) Avoid specific names having, etymologically, the same meaning as the generic name.

Art. 37. Hybrids whose origin has been experimentally demonstrated are designated by the generic name, to which is added a combination of the specific names of the two species from which they are derived, the name of the species that has supplied the pollen being placed first with the final *i* or *o*, and that of the species that has supplied the ovulum coming next, with a hyphen between (*Amaryllis vittato-reginæ*, for the Amaryllis proceeding from *A. reginæ*, fertilized by *A. vittata*.

Hybrids of doubtful origin are named in the same manner as species. They are distinguished by the absence of a number, and by the sign × being prefixed to the generic name (× *Salix capreola*, Kern.).

Art. 38. Names of subspecies and varieties are formed in the same way as specific names, and are added to them according to relative value, beginning by those of the highest rank. Half-breeds (*mules* of florists) of doubtful origin are named and ranked in the same manner.

Subvarieties, variations, subvariations of uncultivated plants may receive names analogous to the foregoing, or merely numbers or letters, for facilitating their arrangement.

Art. 39. Half-breeds (*mules* of florists) of undoubted origin are designated by a combination of the two names of the subspecies, varieties, subvarieties, etc., that have given birth to them, the same rules being observed as in the case of hybrids.

Art. 40. Seedlings, half-breed of uncertain origin, and sports should receive from horticulturists fancy names in common language, as distinct as possible from the Latin names of species or varieties. When they can be traced back to botanical species, subspecies, or variety, this is indicated by a succession of names (*Pelargonium zonale*, Mrs. Pollock).

Section 3. *On the Publication of Names, and on the Date of each Name or Combination of Names.*

Art. 41. The date of a name or of a combination of names is that of its actual and irrevocable publication.

Art. 42. Publication consists in the sale or the distribution among the public of printed matter, plates, or autographs. It consists, likewise, in the sale or the distribution, among the leading public collections, of numbered specimens, accompanied by printed or autograph tickets, bearing the date of the sale or distribution.

Art. 43. The communication of new names in a public meeting, and the placing of names in collections or in gardens open to the public, do not constitute publication.

Art. 44. The date put to a work is presumed to be correct, till there is evidence to the contrary.

Art. 45. A species is not looked upon as named unless it has a generic name as well as a specific one.

Art. 46. A species announced in a work under generic and specific names, but without any information as to its characters, cannot be considered as being published. The same may be said of a genus announced without being characterized.

Art. 47. Botanists will do well to conform to the following recommendations :—

(1.) To give accurately the date of publication of their works or portions of works, and that of the sale or the distribution of named and numbered plants.

(2.) To publish no name without clearly indicating whether it is that of an Order or of a tribe, of a genus or of a section, of a species or of a variety,—in short, without giving an opinion as to the nature of the group to which the name is given.

(3.) To avoid publishing or mentioning in their works unpublished names which they themselves do not accept, especially if the authors of such names have not expressly authorized them to do so. (See Art. 36, 5.)

Section 4. *On the Precision to be given to Names by the Quotation of the Author who first published them.*

Art. 48. For the indication of the name or names of any group to be accurate and complete, it is necessary to quote the author who first published the name or combination of names in question.

Art. 49. An alteration of the constituent characters, or of the circumscription of a group, does not warrant the quotation of another author than the one that first published the name or combination of names. When the alteration is considerable, the words: *mutatis charact.*, or *pro parte*, or *excl. syn.*, *excl. sp.*, *excl. var.*, or any other abridged indication, are added to the quotation of the original author, according to the nature of the changes that have been made, and of that group that is dealt with.

Art. 50. Names published from a private document, such as an herbarium, a non-distributed collection, etc., are individualized by the addition of the name of the author who publishes them, notwithstanding the contrary indication that he may have given. In like manner names used in gardens are individualized by the mention of the author who first publishes them.

The herbarium, the collection, or the garden, should be fully quoted in the text. (*Lam. ex Commers. ms. in Herb. Par.; Lindl. ex horto Lodd.*)

Art. 51. When a group is moved, without alteration of name, to a higher or lower rank than that which it held before, the change is considered equivalent to the creation of an entirely new group, and the author who has effected the change is the one to be quoted.

Art. 52. Authors' names put after those of plants are abbreviated, unless they be very short.

For this purpose, preliminary particles or letters that do not, strictly speaking, form part of the name, are suppressed, and the first letters are given without any omission whatsoever. If a name of one syllable is long enough to make it worth while to abridge it, the first consonants only are given (*Br.* for Brown);

if the name has two or more syllables, the first syllable and the first letter of the following one are taken; or, the two first, if they are both consonants (*Juss.* for De Jussieu; *Rich.* for Richard).

When it is found necessary to give more of a name, for the sake of avoiding confusion between names beginning with the same syllables, the same system is to be followed. For instance, two syllables are given, together with the one or two consonants of the third; or else one of the last characteristic consonants of the name is added (*Bertol.* for Bertoloni, so that it may be distinguished from Bertero; or *Michx.* for Michaux, to prevent confusion with Micheli). Christian names or accessory designations, serving to distinguish two botanists of the same name, are abridged in the same way (*Adr. Juss.* for Adrien de Jussieu, *Gærtn. fil.* or *Gærtn. f.* for Gærtner son).

When it is settled custom to abridge a name in another manner, it is best to conform to it (*L.* for Linnæus, *St.-Hil.* for Saint-Hilaire).

Section 5. *On Names that are to be retained where a Group is divided, remodelled, transferred, or moved from one rank to another, or when two Groups of the same rank are united.*

Art. 53. An alteration of characters, or a revision carrying with it the exclusion of certain elements of a group or the addition of fresh ones, does not warrant a change in the name or names of a group.

Art. 54. When a genus is divided into two or more genera, its name must be retained, and given to one of the chief divisions. If the genus contains a section or some other division which, judging by its name or by its species, is the type or the origin of the group, the name is reserved for that part of it. If there is no such section or subdivision, but one of the parts detached contains, however, a great many more species than the others, it is to that part that the original name is to be applied.

Art. 55. In case two or more groups of the same nature are united into one, the name of the oldest is preserved. If the names are of the same date, the author chooses.

Art. 56. When a species is divided into two or more species, if one of the forms happens to have been distinguished earlier than the others, the name is retained for that form.

Art. 57. When a section or a species is moved into another genus, when a variety or some other division of a species is given as such to another species, the name of the section, the specific name or that of the division of the species, is maintained, unless there arise one of the obstacles mentioned in Articles 62 and 63.

Art. 58. When a tribe is made into an Order, when a subgenus or a section becomes a genus, or a division of a species becomes a species, or *vice versâ*, the old names are maintained, provided the result be not the existence of two genera of the same name in the Vegetable Kingdom, two divisions of a genus, or two species of the same name in the same genus, or two divisions of the same name in the same species.

Section 6. *On Names that are to be rejected, changed, or altered.*

Art. 59. Nobody is authorized to change a name because it is badly chosen or disagreeable, or another is preferable or better known, or for any other motive, either contestable or of little import.

Art. 60. Every one is bound to reject a name in the following cases:—

(1.) When the name is applied, in the Vegetable Kingdom, to a group that has before received a name in due form.

(2.) When it is already in use for a class or for a genus, or is applied to a division or to a species of the same genus, or to a subdivision of the same species.

(3.) When it expresses a character of an attribute that is positively wanting in the whole of the group in question, or at least in the greater part of the elements it is composed of.

(4.) When it is formed by the combination of two languages.

(5.) When it is in opposition to the rules laid down in Section 5.

Art. 61. The name of a cohort, subcohort, Order, suborder, tribe, or subtribe, must be changed if taken from a genus found not to belong to the group in question.

Art. 62. When a subgenus, a section, or a subsection passes as such into another genus, the name must be changed if there is already, in that genus, a group of the same rank, under the same name.

When a species is moved from one genus into another, its specific name must be changed if it is already borne by one of the species of that genus. So likewise when a subspecies, a variety, or some other subdivision of a species is placed under another species, its name must be changed if borne already by a form of like rank of that species.

Art. 63. When a group is transferred to another, keeping there the same rank, its name will have to be changed if it leads to misconception.

Art. 64. In the cases foreseen in Articles 60, 61, 62, 63, the name to be rejected or changed is replaced by the oldest admissible one existing for the group in question; in the absence of this, a new one is to be made.

Art. 65. The name of a class, of a tribe, or of any other group above the genus, may have its termination altered so as to suit rule or custom.

Art. 66. When a name derived from Latin or Greek has been badly written or badly constructed, when a name derived from that of a person has not been written consistently with the true spelling of that name, or when a fault of gender has carried with it incorrect terminations of the names of species or of their modifications, every botanist is authorized to rectify the faulty names or terminations, unless it be a question of a very ancient name current under its incorrect form. This right must be used reservedly, especially if the change is to bear upon the first syllable, and, above all, upon the first letter of the name.

When a name is drawn from a modern language, it is to be maintained just as it was made, even in the case of the spelling having been misunderstood by the author, and justly deserving to be criticized.

Section 7. *On Names of Plants in Modern Languages.*

Art. 67. Latin scientific names, or those that are immediately derived from them, are used by botanists preferably to names of another kind, or having another origin, unless these are very intelligible and in common use.

Art. 68. Every friend of science ought to be opposed to the introduction into a modern language of names of plants that are not already there, unless they are derived from a Latin botanical name that has undergone but a slight alteration.

Editorial Remarks and Suggestions.

We have reprinted this code in full, so that American naturalists generally may be informed by it, and contribute their part, if so disposed, to the discussion which probably is hardly yet closed. We commend to their attention the entire publication, issued by L. Reeve & Co. (at 2*s.* 6*d.* sterling), with its interesting Preliminary Notice, Historical Introduction, and the elaborate Commentary which of itself occupies more than half of the pamphlet. The considerable space which the code alone occupies in our restricted pages prevents any extended commentary of our own. It seems to be generally understood that the reference of this subject to the recent Botanical Congress at Paris was in some sort the result of a criticism made in this Journal (for January, 1867) upon the course followed in a matter of nomenclature by a valued contributor to the latest issued vol-

ume of DeCandolle's Prodromus. The innovation had to be the more pointedly condemned because the work of a most excellent botanist, incorporated into the standard publication of the time, and also the culmination of a tendency for a long while manifest in various quarters; but which, although commonly inspired by justifiable motives, and even claiming the highest accuracy, needed only to be consistently carried out in practice to show its harmfulness and ensure its reprobation. The objectionable plan is explicitly condemned in the present code (Art. 48, 49, 53), both in the original draught by DeCandolle for presentation to the Parisian Congress, and as adopted by that body.

The whole code as here printed, after careful discussion and revision in committee, and in several sittings of the Congress, was submitted to a final vote, in which the "following decision was carried all but unanimously, and with manifest satisfaction, by about a hundred botanists of all countries," viz: "That these Laws, as adopted by this assembly, shall be recommended as the best guide for nomenclature in the Vegetable Kingdom."

The few dissenting voices were probably given mainly against Art. 48, &c., and in reference to the long debated point as to the name which should be cited as authority for a species transferred from its original genus to some other. Whether, for example, if we adopt Pursh's genus *Chimaphila* we shall write "*C. maculata* Pursh," or "*C. maculata L.*", with or without an added "sp.", or "*sub Pyrola*," or whatever else may be devised for incorporating synonymy or history with the binomial appellation and the authority for it. The code enjoins the orthodox practice, and the commentary, after an extended presentation of the arguments on both sides, ably vindicates the rule, as did Agassiz long ago, in his *Nomenclator Zoologicus*. We maintained the same view in this Journal many years ago, and also recently (March, 1864), and have nothing to add, except to express the hope that the British Association, which allowed its Committee in 1842 to propose the innovation complained of, will at its ensuing meeting reconsider its recommendation. Some of the arguments on the other side are worthy of serious consideration; still we are convinced, as are most botanists of long experience, that the evils of the proposed innovation, in any of the forms suggested, far outweigh its possible advantages.

Upon Art. 15, the commentary properly explains that its terms must not be held as an impediment to quoting Tournefort [as Linnæus himself was accustomed to do] or any other for a generic name made before Linnæus and adopted by him. To decline to recognize generic, any more than specific names

before Linnæus, or even those of his contemporaries whom Linnæus himself cites, may no doubt save some trouble and research; but if the authority of Linnæus be justly deferred to, so may his example.

As to Art. 21, 22, to insist that ordinal names made from generic shall uniformly terminate in *-aceæ*, or at least in *-ineæ* when euphony suggests that form, may be somewhat arbitrary; but we approve the rule as laid down, and wish it were followed whenever no good reason appears to the contrary.

The concluding recommendation under Art. 25, is probably worthy of attention: but Art. 60 declares, by implication, that no generic name in botany is to be discarded because of prior use in zoology. Names in both kingdoms are far too numerous, and the obstacles to free acquaintance with them far too great, to require botanists to look after zoological names, or vice versa. It is quite as much as zoologists can do to avoid the use of the same name in two or more classes of the Animal Kingdom.

It was doubtless an oversight, as the commentary suggests, through which, in Art. 33, specific personal names are *required* to have a genitive form in one case and an adjective form in the other: a recommendation merely must have been intended. It is by no mistake, however, that all such names, as well as substantive proper names of species, are required to be written with a capital letter. Some American zoologists, preferring uniformity to sense or taste, write all specific names whatever with a small initial: but the example has hardly ever been followed in botany. The code omits all mention of geographical specific names, being adjectives, such as *Canadensis*, *Pennsylvanica*, and the like, about which the custom differs in different languages, the English language requiring a capital initial. They have this also in DeCandolle's Prodromus, except in the latest volumes; but there is at present a tendency to drop the capital letter. This is a matter of usage.

We are not prepared to accede to Art. 50, even as explained and qualified in the commentary, without which it would scarcely be fully understood. For instance, there may be no necessity for taking up one of Commerson's names affixed by him to his plants in herbaria; but if taken up, simple verity would seem to require this botanist's name to be cited. We should feel bound to write "*Flacourtia* Commerson" although published by L'Heritier or Jussieu, who probably supplied the character. The rule as proposed would apply to names communicated with manuscript characters by one botanist to another, as well as to named specimens. Now, no botanist is bound to do the work of publication for another; but if he chooses to do so, the maxim *qui facit per alias*, etc., must

fairly apply, and succeeding writers should not be required to take the godfather for the father. If we rightly understand the editor, he proposes that we should write *Eulophus*, *Leptocaulis*, and *Trepocarpus*, DC., although the elder DeCandolle, accepting these names with the specimens from Nuttall, scrupulously attributes them to "*Nutt. in litt.*" To us all such names, which the elder DeCandolle has, at his own discretion, published for Nuttall, are of "*Nutt. in DC. Prodr.*" &c.

The Linnæan canon that *hybrid* names, part Greek part Latin, are inadmissible, having properly been kept up (Art. 60, 5,) M. DeCandolle, in the commentary, calls attention to its habitual violation by botanists in the case of personal names, supposed to be Latinized, and which at least are not Greek, to which the Greek *-oides*, *-ides*, or *-opsis*, is added. In his original draft he inclined to reject all such names, hardly considering what numerous changes would be involved; in the present commentary he concedes that, as they are not really Latin with Greek endings, they must be permitted to stand, though faulty. It would surely never do to change *lobelioides*, *rapunculoides*, *campanuloides* (Italian and Greek), *ranunculoides* (good Latin and Greek), *scirpoides* and the like, so freely used by all botanists, from Linnæus to the present day: even A. DeCandolle himself, who hopes he has none of the sort to reproach himself with, gave to a section of *Wahlenbergia* the name of *Lobelioides*. As to specific names formed of *-oides* added to personal generic names of modern and unclassical origin, they could not now be dispensed with; so we must needs insist, with such reason as we may, that the main word is not really Latinized but Grecified; while in case of real Latin words the Latin form of adjunct is not only the proper one but often the more euphonious,—e. g. *ranunculina*, *scirpina*, &c.

This digest, as a whole, is to be highly commended, and it cannot fail to be useful. Its greatest value is this, that it does not make, but only declare, the common law of botanists. Our *Phænogamous* botanists in this country did not need it in the way of correction. Some of the *Cryptogamists* do, and many zoologists.

The English translation is by Dr. Weddell, who states that he has adhered as literally as possible to the original text. In some parts of it the neat French might possibly have been rendered in more terse and idiomatic English with no sacrifice of clearness or accuracy. A. G.

Art. X.—*On the Sulphates of Oxyd of Antimony;* by W. P. Dexter.

The various degrees of saturation of acids with the basic oxyds have been considered, either in general as different classes of *salts*, or have been studied in detail, as combinations of the individual bases. The recent Memoir of Hr. Schultz, of Berlin,* has added largely to the number of compounds known as acid salts. In a work now nearly finished, I hope to show that, of the alum-forming bases, the sulphates of alumina and sesquioxyd of chrome, are capable of combining with an equivalent of hydrated sulphuric acid; that these acid-sulphates stand in close connection with the so-called double salts of the neutral sulphates of these bases; and that they may be regarded as compound acids, of which the double sulphates are the derivative salts. With the view of extending this inquiry to the salts of an oxyd of similar composition, but of a different class, the following examination of the combinations of sulphuric acid with oxyd of antimony was undertaken. No compound was here found analogous to the acid-sulphates of the alum bases; as no salt of oxyd of antimony is known which has the composition of the alums. These compounds have been previously investigated by Brandes, and more recently, and with somewhat different result, by M. Péligot. My own conclusions agree, as will be seen, with those of the German chemist; but are given with hesitation, from experience of the difficulties with which the preparation of these bodies is attended.

For the purpose of demonstrating their existence, and examining their form under the microscope, the salts are easily made by boiling oxyd of antimony or Algaroth powder with dilute sulphuric acid, until the water of the acid is expelled. The bisulphate remains as a white sandy powder, while the concentrated acid deposits on cooling acicular crystals of the ter-sulphate or neutral salt. By the action of hot water, these are converted into minute crystals of the di-sulphate. If the concentration of the acid have not been carried so far, intermixed with, or in place of, the bisulphate will be found crystals of an intermediate salt, of different form: and the liquid, if allowed to attract moisture from the air, deposits still other crystalline compounds, one of which contains equal equivalents of acid and base.

These are the salts which I have succeeded in obtaining in a sufficiently pure state for analysis. They are all crystalline,

* Pogg. Ann., cxxxiii, 137.

dissolve readily in hydrochloric acid, and, with the exception of the disulphate, are decomposed by the action of water. To free them from the adhering acid liquid, they were left for a considerable time upon a porous earthen plate, protected from moisture by a glass containing a vessel of concentrated sulphuric acid. For the plates nothing was here to be found better than the red earthen saucers upon which flower-pots are placed. I had also flat discs made of the same ware, between which the salts, when partially dried, were forcibly pressed by means of a steel screw. The plates, before use, were heated, first in an oven, and then over a large gas flame, or an anthracite fire, and left to cool in a close vessel, over sulphuric acid.

For analysis the salts were dissolved in hydrochloric acid, tartaric added so that the solution could be largely diluted without precipitation, and the antimony thrown down by sulphydric acid: the sulphuric acid was then determined in the filtrate as sulphate of baryta. If, on the contrary, the sulphuric acid be precipitated before the separation of the antimony, a sulphate of baryta is obtained, which after ignition is colored, and retains traces of the metal, that cannot be wholly removed by digestion with dilute hydrochloric acid. All the tartaric acid of commerce, which I have examined, contains sulphuric acid; digestion of the solution in alcohol with a little carbonate of baryta removes the acid, but the tartrate of baryta is not quite insoluble in the alcoholic liquid. With carbonate of lime, no better result was obtained. Through the kindness of Mr. Melvin, druggist of this city, I received a supply of tartaric acid, in large crystals, which were quite free from sulphuric acid and all metallic impurity.

The precipitated sulphid of antimony was collected on a weighed filter, and dried at the temperature of boiling water; from its weight the quantity of the metal or oxyd was calculated. As the antimony was in the state of oxyd, and no nitric acid, oxyd of iron, or other substance capable of decomposing the sulphydric acid, was present, an excess of sulphur in the precipitate was not to be feared. It has been asserted that the tersulphid of antimony retains at 100° C. a quantity of water, not amounting to one p. c., and requiring for its expulsion a temperature above 200°, by which the sulphid is converted into the black, crystalline modification. To determine the precise amount of the error from this source, as well as from the possible presence of uncombined sulphur, the sulphid, after remaining many hours in the water bath, and until its weight varied, at most, one or two tenths of a milligram, was heated in a covered crucible, in an air bath, to 212°—220°, for half or three quarters of an hour at a time, with the following result:

0·8192 SbS_3	lost	2·1	0·4	0·5
0·5445 "	"	1·	0·3	milligrams,

no constant weight being obtained.

The sulphid was then exposed to the same temperature, in a bulb blown upon a narrow glass tube, while the air was displaced by a slow stream of carbonic acid.

0·8536	lost	0·2
0·5199	"	0·5
0·6488	"	0·4
0·3920	"	0·25 milligrams,

these being all products of different analyses; the sulphid was completely converted into the black modification, and the weight on repeated trial remained unchanged. The error, whatever may be its source, of at most the tenth of one p. c., becomes insignificant when compared with the tenfold greater errors inevitable in the preparation of the salts.

Tersulphate, or Neutral Salt.—Oxyd of antimony and Algaroth powder are dissolved in considerable quantity by hot concentrated sulphuric acid, the latter with disengagement of hydrochloric acid, the solvent power of the acid seeming to increase with its temperature. On cooling, a salt is deposited in slender needles, in such quantity, when the acid is saturated at its boiling point, that the whole becomes a thick semi-fluid magma. The crystals are long, four-sided prisms, with terminal faces, set often upon two opposite sides, alike at both ends of the prism. They seem to belong to the oblique rhombic system. The concentrated acid, in which they have formed, retains so little of the oxyd in solution, at the ordinary temperature, that it remains clear when diluted with water; but by sulphydric acid a slight precipitate is produced. In the acid, diluted with about half its volume of water, the oxyd in the cold is much more soluble.

In the preparation of the salt, the oxychlorid was used in preference to the oxyd, as the latter is not easily obtained, free from alkali; and the acid was heated until all water was expelled, and vapor of hydrated acid abundantly given off. The semi-fluid mass of crystals was brought upon a funnel, the neck of which was imperfectly closed by a glass rod, and they were then further freed from the acid upon the porous plates, as has been described. These operations were performed as rapidly as possible, in frosty winter weather, and in a room which was not heated, and was entered for no other purpose. When dry, the salt formed a mass of fibrous texture, very much resembling asbestos.

My analysis shows this to be the tersulphate, or neutral salt:

1. 0·5933 of the salt gave 0·3648 SbS_3:
2. 0·6792 of the same preparation gave 0·4173 SbS_3, and 0·918 $\dot{B}a\dddot{S}$.
3. 0·6413 of another preparation gave 0·3999 SbS_3, and 0·0859 $\dot{B}a\dddot{S}$.

	Calculated.	1.	2.	3.
SbO_3	54·94	52·82	52·78	53·58
$3SO_3$	45·06	——	46·41	46·00
	100·00		99·19	99·58

The difference in the numbers found in the analyses, from those given by calculation, is to be ascribed to impurity of the preparations, rather than to analytical errors. It is obviously difficult, by pressure between rigid plates, to remove completely all adherent liquid, and an excess of acid was therefore generally found in the salts thus prepared. That this was in the present case the cause of the discrepancy, will appear when it is considered that the acid which adhered to the salt was hydrated acid, and that its water would be represented in the analysis by a loss. The loss, then, should correspond in amount to the excess of acid obtained; or, reckoned in equivalents as water, should be equal to the equivalents of the acid in excess.

The composition of the salt from the above analyses, taken in equivalents, is

	2.	3.
SbO_3	1·	1·
SO_3	3·22	3·14
HO (loss)	0·25	0·13

The earlier analyses gave

	Brandes.	Péligot.*	
SbO_3	56·4	50·2	44·3
SO_3	43·2	51·9	53·1
	99·6	102·1	97·4

Mr. Péligot considers the salt to contain four atoms of acid, a composition which requires 47·77 SbO_3 to 52·23 SO_3, if the equivalent of antimony be taken, as has been here done, at 122·34.† The question is one of considerable importance; for from the composition which he has assigned to this sulphate, and to the other antimonial compounds described in his Me-

* The requisite data for calculation are given only for the first of the analyses of M. Péligot. If there be not a misprint, there is an error in this calculation, as occurs also in three out of the four analyses of the sulphates of antimony, of which the details are given. If from 1·66 of the salt, 2·22 $\dot{B}a\dddot{S}$ were obtained, as stated in the memoir, there were 45·9 p. c. of acid, instead of 51·9.

† Pogg. Ann, c, 563; Proceedings of the Am. Acad., Jan. 1862.

moir, Mr. Péligot infers that the salts of antimony, like those of uranium, depart from the recognized law, according to which oxyds containing three equivalents of oxygen require the same number of atoms of acid to form a neutral salt. The existence of such a sulphate of oxyd of antimony is denied by him. "En effet il ne m'a pas été possible de rencontrer un seul sel contenant ·3 équivalents d'acide combiné avec 1 équivalent d'oxyde d'antimoine; de sorte que pour les sels d'antimoine, de même que pour les sels jaunes d'urane, toutes les formules calculées dans les tables d'équivalents de M. Berzelius représent des sels qui n'existent pas."*

But the neutral salt may be prepared in another way, with a composition agreeing so nearly with that required by theory, as to leave no doubt of its existence. By careful heating, in a crucible, of the oxyd, or oxychlorid of antimony, with sulphuric acid, until the excess of acid is expelled, a white residue is obtained, the weight of which, however, is not quite constant, a minute quantity of acid being continually given off, probably on account of the moisture of the air. To obviate this source of uncertainty, the oxyd, or oxychlorid was put into a bulb blown on the end of a narrow and thick glass tube, bent in the shape of a retort, close to the bulb. The neck was then drawn out so as to end in a point, in which, during the distillation, a drop of acid remained, and deprived of its moisture any air which might pass through it. A platinum crucible, lined with asbestos, served as a bath, and was covered with a sheet of mica, perforated for the passage of the neck, over which asbestos was also heaped. The heat was such as just to cause the acid to distil slowly over, the distillation from between one and two grams of oxychlorid lasting several hours: the temperature was above the melting point of lead. At the end, a minute drop collected in the point, at intervals of from a quarter to half an hour. By too long continuance of the heat the vapor of water-free acid is seen in the tube. The acid in the neck having been driven off, the point was sealed with the blowpipe. The product was a coherent, friable mass, crystalline on the surface to the naked eye.

Of a preparation made from the oxyd, 0·4471 gave 0·2823 SbO_3 and 0·5953 $\dot{B}a\dddot{S}$: of a salt made from Algaroth powder, 0·6177 gave 0·3946 SbS_3 and 0·815 $\dot{B}a\dddot{S}$: in the hundred,

	Calculated.	1.	2.
SbO_3	54·94	54·24	54·88
SO_3	45·06	45·72	45·31
	100·00	99·96	100·19

* Ann. Ch. Phys., 3d Ser., xx, 297.

Basic Salts.—The basic sulphates of antimony form a series of salts in which the oxyd is combined with two, one, and half an equivalent of acid. Besides these, there appear to be others, only one of which I have examined, and which are probably combinations of these simpler salts. They occur under various and often unexpected conditions; but, in general, the degree of dilution of the acid seemed to have the greatest influence in their production. When oxyd of antimony is boiled with sulphuric acid diluted with about its volume of water, a turbid liquid is obtained, in which, under the microscope, no distinctly crystalline body can be perceived. On continuing the ebullition, when the acid has reached a certain degree of concentration, the liquid, provided more oxyd be present than it can dissolve, becomes suddenly clear from the subsidence of a heavy sandlike body. If the boiling have been stopped as soon as this takes place, the substance will be found under the microscope to consist of flat, rhombic prisms, of considerable size. By continuing the boiling, but for a few seconds, these disappear, without any change of appearance perceptible to the eye; but by the microscope, the prisms are seen to be replaced by octahedral crystals. Generally, however, the prisms are from the first mingled with the octahedral salt, from which it is very difficult in this way to obtain them quite free. By a slight concentration of the acid, as has been said, the octahedra are left perfectly free from the prisms.

These octahedra appear to belong to the regular system, are frequently more or less distorted, but very seldom show any replacement or modification of the crystalline form. The faces are striated, and to these little projecting angles, acting like prisms, is due, it seems to me, the slight amount of color which they exhibit in polarized light. As the liquid in which they have formed cools, crystals resembling those of the neutral sulphate generally separate. To prevent admixture with these, the porous plate upon which they were collected was covered and imbedded in hot sand. In their analysis were obtained—

1. From 1·1663 salt, 0·863 SbS_3 and 1·1815 $\dot{Ba}\dddot{S}$.
2. From 1·1359 of another preparation, 0·847 SbS_3 and 1·1724 $\dot{Ba}\dddot{S}$.

	Calculated.	1.	2.
SbO_3	64·65	63·57	63·91
$2SO_3$	35·35	34·79	35·44
	100·00	98·36	99·35

The salt is the bisulphate, and its formula $\dddot{Sb}\dddot{S}_2$.

Mr. Péligot, by the action of fuming sulphuric acid upon

oxyd of antimony, obtained a salt "in the form of small, brilliant crystals," of which he has given the analyses:

SbO_3	63·0	64·3
SO_3	37·1	35·0

and which therefore seem to have been the salt just described. In fact, hydrated sulphuric acid, gently warmed, unites with the oxyd to form this compound, which is then dissolved at a higher temperature, with production of the neutral salt.

In the formation of both the octahedral and prismatic crystals, there appears to be direct conversion of the undissolved oxyd into the salts: they are also deposited from a solution of the oxyd in sulphuric acid of the proper concentration. There is, however, a peculiarity attending their formation. When they have separated from the acid liquid, they can be decomposed by addition of water, and again reproduced by concentrating the liquid. But if by still further evaporation they have been redissolved in the acid, as neutral salt, or if the oxyd be at first dissolved in concentrated acid, and the solution in either case decomposed by water, on evaporation the octahedral crystals will not be obtained, or will be obtained only in small quantity: the separated basic salt redissolves at last completely in the acid. By arresting the evaporation at the proper period, as has been said, octahedral crystals may be deposited from the liquid on cooling, but no conversion of the basic salt into them seems to take place. By decomposing the basic salt with carbonate of soda, and treating the resulting oxyd with dilute acid, the octahedral crystals can be again produced.

Finding it difficult to obtain the salt mentioned as occurring in flat prisms free from the octahedral crystals by concentration of the acid liquid in an open vessel, experiments were made with acid of various degrees of dilution. An acid of sp. gr. greater than 1·6, gave, by boiling with the oxyd, octahedral crystals: at 1·597 the prisms first appeared: at sp. gr. 1·57 the oxyd was converted into prisms free from the other salt, but smaller than those produced by the gradual concentration of the liquid; and at 1·554 the product consisted only of needles. So that the production of the body in question is confined within narrow limits of concentration of the acid. The salt was dried, like the bisulphate, upon a plate surrounded with hot sand.

Of two different preparations

0·624 salt gave	0·4864 SbS_3	and	0·571 $\dot{Ba}\dddot{S}$:	(1)
0·524 " "	0·4097 SbS_3	and	0·479 $\dot{Ba}\dddot{S}$:	(2)

Whence their composition was

	1.		2.	
		eqvts.		eqvts.
SbO_3	66·97	3	67·17	3
SO_3	31·42	5·16	31·39	5·13
HO (loss)	1·61	1·17	1·44	1·05
	100·00		100·00	

and their formula $\ddot{S}b_3\dddot{S}_5, \dot{H}$, which requires

SbO_3 - - - - - -	67·75
SO_3 - - - - - -	30·86
HO - - - - - -	1·39
	100·00

The salt is most probably a compound of two atoms of bisulphate, and one of protosulphate, $2\ddot{S}b\,\dddot{S}_2 + \ddot{S}b\,\dddot{S}, \dot{H}$.

This composition explains the facility with which it is changed by prolonged boiling, and concentration of the liquid into the bisulphate; it being only necessary that the equivalent of water be replaced by one of acid.

The crystals of this salt appear large and well defined, under the microscope with medium powers. They are four-sided prisms, having for their section a rhomb with very unequal angles, and terminated by two or four faces, which, from the flatness of the prisms, are not distinctly visible: they belong apparently to the right rhombic system. Formed by the gradual concentration of the acid, they have always the same form; but by exposure of the liquid, or of a solution of the oxyd in dilute acid, to the air, crystals are obtained of the same general appearance, which seem to be of the oblique rhombic system.

By exposure for several weeks to the air, of the liquid from which the prisms had separated, a fine white powder was deposited, consisting of minute irregular needles, in which, with a power of about 600, no further shape could be made out. The quantity was only sufficient for one analysis, and a subsequent attempt to form them did not succeed. 0·3447 gave 0.2668 SbS_3 and 0·2167 SO_3.

	Calculated.	Found.
SbO_3	74·92	73·89
SO_3	20·48	21·59
HO	4·60	(4·43)
	100·00	100·00

The same salt, without the atom of water, was made by Brandes by the action of alcohol upon the neutral salt. I

have obtained in this way a salt in small needles, but which was not analyzed.

The last of these basic salts, the combination of two atoms of oxyd with one of acid, has been described and analyzed, both by Brandes and by Péligot. It is produced by the decomposition by water of the neutral salt, or its solution in dilute sulphuric acid. Precipitated from a solution, I have found it amorphous, but by standing two or three days in contact with the liquid, it crystallizes in needles. According to Brandes, the amorphous salt loses the greater part of its acid by washing with water. The neutral salt, or rather the magma to which the solution of oxyd of antimony in concentrated acid congeals on cooling, is resolved into minute crystalline needles, when decomposed by warm water, or when by the dilution of the acid sufficient heat is evolved. They subsided readily in the liquid, and could be boiled with repeated portions of water without changing their appearance, or the production of an amorphous substance. When brought upon a filter and washed copiously with boiling water, the filtrate contained constantly a little sulphuric acid and deposited oxyd of antimony on cooling. From the agreement of the result of their analysis with the calculated composition, it would seem that in the crystalline state, this salt is not decomposed, or is but slowly decomposed by hot water. 0·8334 of the salt prepared in this way, and dried by pressure in paper, gave 0·8393 SbS_3 and 0·2735 $\dot{B}a\overline{S}$.

	Calculated.	Found.
$2SbO_3$	85·66	86·52
SO_3	11·71	11·27
HO	2·63	(2·21)
	100·00	100·00

Brandes found in the salt 3 p. c. of water. Péligot obtained it water free, and also with two atoms of water. Heated to 100° it lost one half p. c.; the rest of the water required for its expulsion a temperature above 240°.

The series of the sulphates of antimony resembles those of the earths glucina and zirconia, considered as sesquioxyds. The neutral sulphate of neither of these earths combines with hydrate of sulphuric acid to form an acid sulphate,* and the most basic compound of both contains two equivalents of base to one of acid. In place of a bisulphate, they have salts with three atoms of acid to two of the earths, and no intermediate salt, like that of antimony, has yet been discovered. From

* Zirconia, Berzelius, but perhaps not quite certain—glucina, Berzelius, and my own experiments.

bismuth the antimony series differs in that the salts of the former metal are decomposed by water, according to Heinz and Ruge, into basic salts containing equal equivalents of acid and base ; here, also, no immediate salt is yet known. A more important difference lies in the fact that oxyd of bismuth appears to form one, if not two, acid sulphates. I have analyses indicating the existence of compounds of the neutral salt with three atoms of hydrate of sulphuric acid; and also of a salt crystrallizing in beautiful pearly scales, and containing equal equivalents of the neutral sulphates of bismuth and potash. A salt with three equivalents of sulphate of potash has been described by Heinz. The further account of these bodies must be reserved until their analyses have given more trustworthy results.

Boston, April, 1868.

Art. XI.—*On the Secular Variation of the Elements of the Earth's Orbit* ; by John L. Stockwell. With a plate.

A knowledge of the elements of the earth's orbit is of great importance in the discussion of many questions of physics which occupy the attention of the scientific men of the present day. Indeed, there are many important problems, whose solution depends entirely upon the values of these elements ; and many others in which these elements are either intimately or remotely connected. As an example, we may refer to astronomical phenomena of eclipses of the sun and moon. Since the intensity of the sun's disturbing force depends, to a certain extent, upon the eccentricity of its orbit, the amount of lunar perturbation depending on the sun's attraction must vary from age to age ; sometimes hastening, and at other times retarding the occurrence of such phenomena.

But it is not alone in astronomical phenomena that the changing elements of the earth's orbit have an important influence. For the intensity of the sun's light and heat follows the same law of variation as the force of gravitation ; and it is evident that the secular variation of the conditions on which these forces depend, must affect the intensity of the forces themselves, and thus have an important bearing on the development of the vegetable and animal kingdoms. Whether the secular variations of the forces of light and heat, in so far as they depend upon the eccentricity of the earth's orbit, are sufficient to account for the changes which geology shows to have taken place during the ages that are past, we do not purpose now to determine. Nor do we purpose to give the necessary

data for determining the secular variation of the climate of either hemisphere of the earth. It would be necessary, for this purpose, to take into account the secular variation of the precession of the equinoxes, and also of the obliquity of the ecliptic to the terrestrial equator, in order to determine the obliquity and intensity of the sun's rays on any given latitude of the earth's surface. But so far as the earth as a whole is concerned, the only element which it is necessary to take into account, is the eccentricity of the orbit. And the values of this element are given in the following table, at intervals of ten thousand years, together with the longitude of the perihelion of the earth's orbit.

The data and formulas on which this table is based, are given in the *Treatise on the Secular Equations of the Moon's Mean Motion*, which we lately published. The materials used in the preparation of the formulas, are the same as those used in the construction of the *American Ephemeris and Nautical Almanac*, with the exception of the mass of the earth, which has been increased to $\frac{1}{321133}$. With those elements and masses we have determined anew the secular variations of the elements of the orbits of the eight principal planets. The constants which we have obtained differ somewhat from those obtained by LeVerrier, in his *Memoir on the Secular Inequalities of the Seven Principal Planets*; not only on account of the disturbing influence of the planet *Neptune*, which had not been discovered at the time of his investigation, but also on account of the improved values of the masses and elements of the other planets. We find, in fact, that the superior limit of the eccentricity of the earth's orbit, which LeVerrier gives as equal to 0·07775, should be reduced to 0·06939; and an increase of the mass of the earth, corresponding to the latest determinations of the solar parallax, would reduce the value of the superior limit still more.

Having thus explained the motives which have determined the computation of the following table, and the authority on which it is founded, it only remains to explain very briefly the accompanying chart and the table on which it is based.

In the *Treatise on Secular Equations*, &c., already referred to, we have given a table and chart, showing the eccentricity of the earth's orbit during the period of a million of years; and the table and chart here given, are merely an extension of that table and chart, over a *preceding* million of years; so that the two tables together extend over a period of two million years. The first date in the following table, therefore, corresponds to 1175566 years before the year 1850, or to 1100000 years before the epoch of the integral, in the *Treatise*

referred to. In the accompanying chart, the perpendicular distance from the line AB, to the curve, shows the value of the eccentricity, at the corresponding dates; the distance of the line A′B′ from the line AB, shows the eccentricity in the year 1850; the line XX′, in like manner, shows the mean value of the eccentricity, or, in other words, the square root of the non-periodic term in the expression of the square of the eccentricity; and lastly, the line A″B″, shows the superior limit of the eccentricity. The unit of abscissas is equivalent to 10000 years; that of the ordinates is equivalent to 0·001.

A simple inspection of the charts indicates a curious relation, that could not be so readily discovered by studying the tabular numbers; and that is, that the points of maxima and minima are approximately repeated at intervals of 1,450,000 years. In other words, if at any epoch, there is a maxima or minima of any given magnitude, in 1,450,000 years before or after that epoch, there will be a corresponding maxima or minima. Referring to the charts for a particular example, we notice that for the abscissa −77, there is a very large maxima, and equal to 0·066 nearly; and if we add 145 units of abscissa to −77, we get +68, and for this abscissa the corresponding ordinate, or the eccentricity, is equal to 0·058. The maxima which immediately precede the ones here referred to, are respectively 0·061 and 0·053. The preceding relation seems to hold good for the twelve points of maxima and minima that are repeated within the period over which the computation extends. The longitudes of the perihelion of the earth's orbit are given, for the purpose of showing the great irregularity in motion of that element, and are referred to the equinox and ecliptic of 1850.

We are not aware that any computation of a similar nature has been performed by any one, nearly to the extent here given. The only one at all similar, which we remember having seen, was published by Croll a few years ago, and was derived from LeVerrier's formula. But the intervals used in his computation were much too long, being 50,000 years, instead of 10,000 years, which we have used in our computation. It is evident, by inspection of the chart here given, that many points of maxima and minima would escape our notice, were we to attempt to chart the curve by means of every fifth ordinate.

It is hoped that the following results may be found serviceable in the discussion of those problems of terrestrial physics, in which solar influence, (whether heat, light, or gravitation,) acts as a controlling agent.

Table showing the Elements of the Earth's Orbit during a period of one Million of Years.

t.	Longitude of Perigee. w'.	Eccentricity. e'.	t.	Longitude of Perigee. w'.	Eccentricity. e'.
−1100000	11° 54′	0·02130	−590000	35° 49′	0·03679
1090000	76 12	0·01066	580000	74 6	0·03358
1080000	184 11	0·01150	570000	113 42	0·02826
1070000	246 2	0·02104	560000	159 56	0·01970
1060000	287 10	0·02922	550000	232 42	0·01203
1050000	327 32	0·03397	540000	325 41	0.01628
1040000	8 40	0·03509	530000	20 26	0.02962
1030000	52 40	0·03327	520000	61 19	0·04014
1020000	101 40	0·03043	510000	97 48	0·04892
1010000	155 8	0·02915	500000	132 36	0.05324
1000000	208 31	0·03085	490000	166 28	0·05241
990000	256 41	0 03430	480000	199 42	0·04629
980000	299 12	0·03734	470000	232 30	0·03572
970000	337 57	0·03733	460000	264 49	0·02191
960000	14 16	0·03400	450000	295 44	0·00652
950000	49 32	0·02697	445000	139 8	0·00157
940000	86 8	0·01663	440000	150 53	0·00883
930000	149 7	0·00452	430000	181 53	0·02248
920000	303 11	0·01147	420000	213 21	0·03328
910000	342 13	0·02593	410000	244 47	0·04034
900000	14 42	0·03941	400000	276 15	0·04341
890000	45 44	0·05055	390000	308 2	0·04273
880000	76 12	0·05807	380000	340 32	0·03865
870000	106 22	0·06099	370000	14 37	0 03199
860000	136 17	0·05862	360000	52 16	0·02380
850000	165 48	0·05083	350000	98 47	0·01553
840000	194 7	0·03800	340000	168 15	0 01027
830000	217 25	0·02138	330000	246 16	0·01185
820000	183 19	0·00640	320000	302 18	0·01649
810000	134 54	0 02014	310000	346 5	0·01970
800000	158 20	0·03834	300000	29 38	0·02076
790000	172 38	0·05309	290000	76 43	0·01985
780000	218 5	0·06263	280000	128 9	0·01815
770000	249 35	0·06592	270000	190 28	0·01977
760000	281 39	0·06275	260000	245 20	0·02429
750000	314 22	0·05369	250000	291 30	0·03021
740000	348 7	0·04000	240000	331 41	0·03520
730000	25 2	0·02358	230000	7 54	0·03740
720000	85 51	0·00737	220000	41 15	0·03594
710000	233 49	0·01181	210000	71 30	0·03056
700000	279 9	0·02496	200000	96 11	0·02190
690000	315 6	0·03460	190000	101 36	0·01322
680000	349 43	0·03990	180000	63 47	0·01107
670000	24 42	0·04079	170000	61 29	0·02106
660000	61 24	0·03788	160000	81 16	0·03177
650000	94 17	0·03188	150000	105 41	0·04022
640000	148 35	0·02651	140000	130 55	0·04544
630000	204 37	0·02305	130000	155 39	0·04725
620000	262 47	0·02437	120000	184 52	0·04600
610000	313 18	0·02883	110000	200 31	0·04204
−600000	356 27	0·03318	−100000	219 14	0·03669

ART. XII.—*On some Cretaceous fossil Plants from Nebraska;* by L. LESQUEREUX.

THE plants here described have been obtained and sent for examination by Dr. John LeConte of Philadelphia, and by far the largest number by Dr. F. V. Hayden, director of the geological survey of Nebraska. It is by the direction of Dr. Hayden that this paper is prepared as a summary of a more detailed report made for him, which embraces more complete descriptions, with figures of all the species.

All these plants come from the Cretaceous formation north of Fort Ellsworth, Nebraska, or its vicinity.

A small number of splendid specimens from the same formation, belonging to the Smithsonian Institution and obtained by Prof. B. F. Mudge of Manhattan College, were lately sent to me just in time to be described here as a valuable contribution to the flora of the Cretaceous of America. They have been also figured for Dr. Hayden's report.

1. Fragment of a Fern, probably a *Lygodium*. It is too small for determination, even for description, and is merely figured as the only remains of Fern found among the Cretaceous specimens. The veinlets are obsolete; as far as they can be seen, they appear slightly arched and nearly perpendicular to the medial nerve, which branches above the middle of a narrow linear leaf or part of leaf. The medial nerve is thicker than in any *Lygodium* known to me; but its branching from above the base indicates a division of a linear leaf as it is generally seen in species of this genus.

2. *Pterophyllum Haydenii*, sp. nov.—Three parts of the same plant in three specimens, viz: part of a frond, with a pinna, and a cone. The surface of the frond is rugose, bearing a row of dots which look like the scars of the pinnæ. The pinnæ are linear, short, two inches long, half an inch broad, slightly enlarged in the middle, and a little curved upward, and also slightly attenuated at the base and at the round point. They are marked by linear parallel striæ, preserving the same thickness in their whole length and undivided. The cone is cylindrical, formed of large imbricated apparently round scales placed in spiral around the rachis. From the broken specimens it is not possible to get a good idea of the form of the whole plant. But it is evidently closely related to *Pterophyllum Ernestinæ*, Stiehler (Palæont., vol. v, p. 76, tab. xv), a species represented by the same parts of the plant as ours, and which merely differs by slightly broader, more obtuse, straight pinnæ.

3. *Glyptostrobus gracillimus*, sp. nov.—This species resembles *G. Ungeri* Heer, from the Tertiary of Europe, differing by more slender branches and shorter leaves. The branches are thread-like, much divided, the leaves half embracing at the base and either pointed or slightly obtuse. Some of the branches inflated at the point appear to bear male flowers. One of the numerous specimens covered with branches of this little conifer is traversed by a narrow cylindrical cone, of which a few of the scales only are visible. The scales, rhomboidal in outline but pointed at the four corners, are marked in the middle by an oval dot or scar, from which thin, linear closely approached striæ diverge to the borders. It is very probably the cone of this species.

4. *Sequoia formosa*, sp. nov.—A fine cone referred with some doubt to this genus. It is about two inches long, proportionally narrow, spindle shaped, inflated in the middle, and tapering upward and downward about in the same degree, to the point and to the short pedicle. The rhomboidal scales are bordered by an elevated margo, and marked by diverging wrinkles tending to a round point at the top of the scales. No fossil cone of this kind has been published, except one by Prof. Heer in his Urwelt der Schweitz, p. 310, as the cone of *Sequoia Sternbergii* Heer, from the Tertiary. This is nearly round, obtuse, of a very different form. The scales only are of the same shape.

5. *Phyllocladus subintegrifolius*, sp. nov.—A small obovate leaf, attenuated to a short petiole, abruptly rounded and undulate lobed at top. Its substance is pretty thick, subcoriaceous. The medial nerve is narrow; the lateral veins close together, nearly contiguous, are thin, simple or undivided, a few only appearing inflated or stronger than the others. No fossil plant is related to this. By the form of its leaf and its nervation it much resembles *Phyllocladus asplenifolius* of Tasmania.

6. *Arundo cretaceus*, sp. nov.—Merely two small specimens, representing knots of branches or of roots of an Arundo, as they appear when separated from the stem. They are circular button-like scars, more than half an inch broad, elevated in the center, and marked at the borders and near the center by two parallel rows of verrucose, irregular, mostly round convex papillæ. They resemble the scars of *Arundo Göpperti* Heer, from the Miocene of Switzerland. But as the specimens were picked up, detached from the stem, the specification is somewhat uncertain. There is, nevertheless, from the same locality a specimen representing part of a striated stem, 1½ inch broad, which is referable to an *Arundo* or a *Cyperites*. It shows the

double nervation of the stems of this genus; deep, well marked striæ, separated by four or five close, thin lines, scarcely distinguishable with the naked eye.

7. *Liquidambar integrifolius*, sp. nov.—A well preserved, large, and nearly entire leaf. Its form is about the same as that of the leaves of our *Liquidambar Styracyflua*. It is membranaceous, shining, round in outline, deeply five-lobed, with lobes obtusely pointed and *entire*. The primary, secondary and ultimate reticulation is that of our living *Liquidambar*. The petiole of the leaf is destroyed, but it is evidently surrounded by that small basilar subdivision of the leaf which is remarked in our living species. All the fossil *Liquidambar* but this have the borders dentate or serrulate like the species of our time.

8. *Populus Lancastriensis*, sp. nov.—Leaf of a thin substance, with a deeply marked nervation, broadly cordate, probably acute (the point is destroyed(, borne on a slender petiole. Its borders are entire or slightly undulate; its primary nerves in five like the lateral ones are proportionally slender. It is broader and more deeply cordate at its base than any of the species of *Populus* from the Tertiary, resembling by its form a *Dombeyopsis*.

9. *Populites cyclophylla* (*Populus*) Heer.—Leaves round, entire, with slightly undulate borders. Primary nerves in three attached to the petiole; secondary nerves (4 pairs) nearly opposite, running straight to the borders in preserving nearly the same thickness; angle of divergence 40°.—This is apparently Heer's species: *Populus cyclophylla*, of which a short diagnosis is given in Proceed. of Acad. of Nat. Sciences, Philad., 1858, p. 266. The author remarks that the base is narrowed and the leaf orbicular, a description applicable as well to the following species as to this one. The form of the leaves of this species is similar to that of *Populus Griemana* (Ldg.) from the middle Tertiary of the Rhine; but the nervation is far different.

This and the four following species can not be considered as belonging to the genus *Populus*. They differ not only by the narrowed base descending in a curve to the petiole, but especially by the nervation which is truly craspedodrome, the secondary nerves running up to the borders as in the beach. By the nervation, therefore, their relation is rather with *Platanus* than with *Populus*. But the leaves are all entire, oval, or round, or flabelliform. They belong evidently to a new genus whose relation and characters have not yet been thoroughly ascertained.

10. *Populites elegans*, sp. nov.—This species has larger leaves than the former, and also differs from it by its strong, slightly undulate, more divided secondary nerves, and by its more elongated base and long petiole. The number of the secondary nerves and their angle of divergence are the same.

11. *Populites ovata*, sp. nov.—Leaves ovate in outline, with an obtuse or truncate point; borders undulate, enlarging toward the base and abruptly curved to a pretty long slender petiole. The primary nerves are in three or five; the secondary ones are thin, alternate, distant, few in number, with an angle of divergence of 30°. The ultimate reticulation derived from pretty large square subcontinuous areas is polygonal and small, like that of a *Platanus*.

12. *Populites quadrangularis*, sp. nov.—This leaf is about quadrangular in outline, with obtuse angles and convex borders; the upper part undulate, the lower entire. The petiole and medial nerve are slender, the primary nerves in five, the lowest pair at a short distance from the upper, the secondary nerves numerous, six on each side, running parallel and slightly arched to the borders. The substance of the leaves is pretty thick. The lowest primary nerves, shorter and more slender, run along the borders as in *Populus*.

13. *Populites flabellata*, sp. nov.—A round fan-shaped leaf somewhat abruptly narrowed toward the base, petioled and thin. The borders are slightly crenulate, especially in the lower part of the leaf. The medial nerve, strongly curved to one side, is thick as also the lateral and secondary nerves, which are alternate, pretty close to each other and branching. The veinlets are deeply marked, perpendicular to the nerves and continuous.

14. *Populites Salisburiæfolia*, sp. nov.—The general outline of this leaf is about the same as in the former. It is fan-like also, narrowed to the petiole where the borders are slightly decurrent. Its broadest part is above the middle, and the top, nearly flat, is divided by two deep narrow sinuses, in three obtuse, undulate or slightly crenate lobes. The nervation of this species is that of an *Acer*. The primary nerves in three, from above the base of the leaf, are distinct but narrow; the lateral ones ascend to the top of the lobes in dividing outward; the medial one branching from above the middle in opposite arched divisions, ascends to the slightly notched top of the leaf.

15. *Salix proteæfolia*, sp. nov.—Except that the leaves are smooth or polished, apparently thick and coriaceous, this species so much resembles *Salix tenera* Al. Br., that no difference can be pointed out in these forms. The leaves are lanceolate or linear-lanceolate, merely pointed or tapering upward into an

elongated point; sometimes slightly enlarged near the base and gradually narrowed into a thin petiole, more than half an inch long. The secondary veins rarely visible, ascending under an angle of 40 to 45°, curve near the borders and are separated by shorter intermediate ones. In some of the specimens referred to this species, the substance of the leaves appears thinner and the surface not polished. This difference may be due to the nature of the stone in which the leaves have been imbedded, or there may be two species whose leaves are exactly alike in form and nervation. There is still among the specimens one leaf which, though of the same form, is much smaller, obtusely pointed, with a longer petiole. The nervation is totally obsolete. It may belong to another species.

16. *Betula Beatriciana*, sp. nov.—Leaf enlarged above the middle, obovate in outline, cuneiform to the petiole, tapering upward to a point; unequally simply toothed in its upper part. The nervation, general form and surface of the leaves are those of the genus. There are many fragments of the species. On some specimens the teeth appear somewhat obtuse.

17. *Fagus polycladus*, sp. nov.—An oval, probably obtusely pointed leaf (the point is destroyed), narrowed to the petiole, entire and wavy on the borders, with a narrow medial nerve and very slender, numerous (14 to 16 pair) parallel secondary nerves, ascending under an angle of 40° straight to the borders. The substance of the leaves is thin, its surface polished. From the great number of secondary nerves and their proximity, this species resembles *Fagus Deucalionis* Ung.; but this has dentate borders. By its form and its undulate entire borders, it is like *Fagus sylvatica* S., of our time, merely differing by the number of the veins.

18. *Quercus primordialis*, sp. nov.—This species is of a type which has many representatives in the Tertiary, and which is still found most predominant among our living oaks. Its nearest relation is our *Quercus prinoides* Mich., which it so closely resembles as to be undistinguishable from it by any characters taken from the leaves. The leaf is oblong-lanceolate, taper pointed at both ends, short petioled, with borders regularly dentate, the teeth pointing upward, separated by obtuse sinuses. The medial nerve is sharp and narrow like the secondary nerves (about 14 on each side), which run straight to the sharp point of the teeth under an angle of 40°. The veinlets are clearly marked, perpendicular to the nerves and continuous.

19. *Quercus hexagona*, sp. nov.—There is only one broken specimen of this species. In the middle of the leaf the borders are nearly parallel and entire; above they taper to a point and

are marked by a few strong pointed teeth. Downward they are slightly enlarged just above the base, and hence are attenuated to the petiole, giving thus an hexagonal form to the leaf. The secondary nerves, seven on each side, slightly curve in ascending each to the point of a tooth.

20. *Quercus Ellsworthianus*, sp. nov.—A leaf somewhat like the following, resembling *Quercus Lyellii* Heer, by its oblong oval form and undulate borders; the point is destroyed; the base is narrowed to the petiole. The substance of the leaf is thin and the secondary nerves very slender, camptodrome, sometimes branching near the borders.

21. *Quercus anceps*, sp. nov.—Allied like the former to *Quercus Lyellii* Heer. Leaves coriaceous, ovate-lanceolate, pointed, or short-acuminate, attenuated to the base, with very entire, not undulate borders. Secondary nerves strong, camptodrome, curving near and along the borders; veinlets perpendicular, nearly continuous, mostly branching. The base of the leaf is destroyed.

22. *Quercus semi-alatus*, sp. nov.—The leaves of this species are irregular in form and nervation. The general outline is ovate-lanceolate, obtuse at the base and at the top. It is more extended on one side than on the other, obtusely lobed on one side and merely undulate and entire on the other. Secondary nerves alternate, strong, at variable distances, of various length, craspedodrome and camptodrome, diverging under various angles, with the reticulation of a *Platanus*. The substance of the leaves is pretty thick.

23. *Ficus? rhomboideus*, sp. nov.—A remarkable leaf. It is a little broader than long. From its broadest obtuse part in the middle, it is broadly wedge-form to the base, and also to the top, having therefore a broadly rhomboidal form. The borders from the middle downward are entire; obtusely crenulate or undulate upward. The nerves palmately divide in five from the base, the exterior ones following the borders though not quite parallel, and ascending to middle of the leaf; the internal ones ascending in an acute angle nearly to the top, where they curve inward to the medial nerve. This one is of the same thickness, and does not branch but by veinlets. The ultimate meshes of the reticulation are broad and polygonal as in a *Populus*. The affinity of this leaf is still uncertain.

24. *Ficus? fimbriatus*, sp. nov.—This leaf, of which a fragment only is preserved, is still more remarkable than the former. Kidney-shaped in outline, truncate at its broad base, its whole upper border is surrounded by regular, erect teeth, inflated or dilated at the point as if bearing small leafy expansions, and separated by broad obtuse sinuses. The petiole is

thick, attached to the leaf a little above the lower border, which is thus continuous. It is palmately seven-nerved, the nerves scarcely branching, diverging all around and vanishing before reaching the borders. The veinlets are perpendicular to the veins, slightly arched, subcontinuous; ultimate reticulation polygonal, small, like that of a *Ficus*. But the relation with this genus is uncertain.

25. *Platanus aceroides?* Göpp., var. *latior*.—Leaf broader than long, palmately trilobate, with short scarcely distinct lateral lobes; borders distantly dentate, flat or undulate between the teeth, the short teeth like the lobes pointing outward. The primary nerves diverge from the petiole just at the border of the leaf, under an angle of 40° to 50°, like the secondary ones. The base of the leaf is slightly rounded or rather truncate, the petiole thick, the substance of the leaves pretty thick and the surface rough. It may be a distinct species, differing from the more common forms of *P. aceroides* by a comparatively broader leaf, shorter lobes, and teeth pointing outward. But we find the same characters in *Platanus rugosus* Göpp., a Miocenic species which Prof. Heer considers as a mere variety of *P. aceroides*.

26. *Platanus Newberrii* Heer.—There is only one fragment of this species among the specimens sent for examination, and it is about as broken and uncomplete as the one figured by Prof. Heer in Phyllites du Nebraska. It evidently differs from the former by its more narrowed base, the leaf descending to the petiole in a broad wedge-form. The primary and secondary nerves are proportionally narrower and the veinlets more irregular and divided. In spite of these apparent differences, the relation of both species is not positively denied. This leaf, like the former, is pretty thick, coriaceous and of large size.

27. *Platanus obtusiloba*, sp. nov.—This species, represented by many specimens, has still more numerous and marked varieties than *Platanus aceroides*. It is palmately, obscurely, irregularly trilobate, the lobes being sometimes marked merely on one side of the leaves and always more or less obtuse. The borders are rather deeply undulate than obtusely dentate, and the base either cordate or rounded or attenuated wedge-form. The size of the leaves is equally variable. The only general character recognizable on all the specimens is the position of the point of union of the primary nerves, generally in three, at a distance above the base of the leaves or the point of attach of the petiole. Except this, the nerves themselves differ in their mode of branching, the medial one especially,

which is sometimes pinnately and alternately divided and sometimes without any branches.

28. *Platanus diminutivus*, sp. nov.—Though far different in form, size, &c., from the former species, this may be still a peculiar variety of it. It is a very small leaf, no more than one inch long, nearly as broad as long, cordate, obtusely pointed in outline, with wavy borders and round base. The primary nerves are in three, but one of the lateral is lower than the other; all proportionally very thick and inflated here and there. The secondary and tertiary nervation and also the color and substance of the leaves are the same as in the former species.

29. *Credneria LeConteana*, sp. nov.—The leaf, a part of which only is preserved, is large, measuring in its broadest part about seven inches, being apparently somewhat longer. Its form is broadly subcordate, rounded at base, with borders entire. All the nerves and veinlets are deeply marked; the upper secondary nerves diverging at an angle of about 50°, while the two lower ones are shorter, narrower and nearly horizontal. It is the position of these two lower secondary pair of nerves which marks the essential character of the genus *Credneria* (Zenker), as limited by Stiehler; and this character is well distinct in our leaf whose size, outline, &c., corresponds exactly with the species described by Stiehler from the Cretaceous of the Hartz mountains. This species, like *Pterophyllum Haydenii*, points out a relation of our Nebraska Cretaceous with that of Europe.

30. *Laurus Nebrascencis* (*Persœa Nebrascencis* Lesq.), Trans. Amer. Phil. Soc., vol. xiii, p. 431, pl. 23, fig. 940.—A thick, coriaceous, ovate-lanceolate pointed leaf, narrowed downward to a pretty long and thick petiole, with the nervation of a *Laurus* or *Persœa*. From its pretty long petiole and its great likeness to *Persœa speciosa* Heer, I formerly considered this leaf as a *Persœa*. But the fruit described below, of a true *Laurus*, found in specimens from the same locality as this leaf, forces the admission of it being a *Laurus*.

31. *Laurus macrocarpus*, sp. nov.—Fruit of a *Laurus*, probably of the former species. It is a round, oval, pretty large nutlet, somewhat roughened, undulate across, and marked lengthwise by narrow, equidistant, scarcely distinguishable ribs. The support is club-shaped, smooth, marked upon its top, or on the surface where it joins the nut, by four vascular dots. The nut is inclosed into a pretty thick, apparently spongious pericarp, split around, at points corresponding to the horizontal wrinkles.

32. *Sassafras Cretaceus* Newb.—Much like our *Sassafras officinalis* ,from which it would be undistinguishable, indeed, but for the thick, coriaceous substance of the leaves. The leaves taper to the base from an enlarged trilobed top; the lateral lobes, diverging and nearly horizontal, are slightly longer than the medial one, all obtusely pointed. The nervation is very distinct; the primary nerves thicker than in the living species, the reticulation exactly of the same kind.

33. *Sassafras Mudgii*, sp. nov.—Though nearly similar to the former in its general outline, it is evidently a different species. The leaf, which is nearly entire, is 8 inches long, 5 inches broad, at the point of the lateral lobes, gradually narrowing to the petiole, three lobate from the middle. Lateral lobes oblique, not half as much diverging as in the former species, proportionally narrower and more obtusely pointed. The medial lobe is twice as long as the lateral ones; substance of the leaves thinner, slightly membranaceous, polished.

34. *Sassafras subintegrifolius*, sp. nov.—The leaf appears to be trilobate or merely emarginate at its rounded top, broadly oval, tapering in an obtuse angle to the petiole, entire, subcoriaceous. It is palmately triple-nerved; the lateral nerves not being opposite but at a short distance from each other, as it is often the case in the genus *Sassafras*. This species is distantly related to *S. Æsculapi* Heer, of the Swiss Miocene.

35. *Proteoides Daphnogenoides* Heer.—Leaves ovate-lanceolate near the base, tapering upward to a very long, acute, slightly scythe-shaped point, smooth and coriaceous, attenuated downward to a short thick petiole; medial nerve narrow, secondary nerves obsolete, few, ascending under a very acute angle along the borders. These leaves are very long. Our specimens show two nearly whole leaves, one of which is 8 inches long, and only one inch broad near the base. As the species of Prof. Heer in the "Phyllites du Nebraska" are represented by mere small fragments, it is not positively certain that ours is the same.

36. *Proteoides acuta* Heer.—Leaves narrower than in the former, with a longer tapering base, undulate borders, thinner, and not polished on the surface. No trace of lateral nerve distinguishable.

37. *Lyriodendron intermedium*, sp. nov.—A part of a leaf evidently belonging to this genus. The upper segments are comparatively long, obtuse, at a distance from the lower ones, indicating a proportionally long and narrow leaf.

38. *Lyriodendron giganteum*, sp. nov.—A species represented still by a broken fragment. It is only one of the supe-

rior lobes of the leaf, with the sinus which separates the upper segments. It is broadly oval-oblong, very obtuse, thick and coriaceous, with the medial and the three secondary nerves which generally enter the upper lobes of a *Lyriodendron.* This leaf is evidently of very large size.

39. *Magnolia tenuifolia,* sp. nov.—A large leaf of thin texture, 6 inches long, 2½ inches broad, oblong, obtusely pointed?, slightly rounded and attenuated to a pretty long petiole. Secondary nerves narrow, open, angle of divergence about 60°; medial nerve sharply grooved.

40. *Magnolia alternans* Heer.—There is only one specimen recognizable of this species. It is like that of fig. 3, pl. 3, in the *Phyllites du Nebraska.* The leaf is coriaceous, ovate-oblong lancolate, gradually narrowed to the petiole; medial nerve thick, lateral veins ascending in acute angle, curving and anastomosing along the borders.

41. *Dombeyopsis obtusiloba,* sp. nov.—The finest specimen of the whole collection. The leaf is whole, merely deprived of its petiole; 7 inches long, 5 inches broad near the base where it is the broadest. Its general outline is triangular-elongated, rounded subcordate at base, gradually diminishing to an obtuse point, deeply sinuous or obtusely irregularly lobed on the borders. It is palmately seven-nerved at the base, the medial nerve thick, as well as the two proximate ones which ascend to ¾ of the leaf in an acute angle; the ultimate or basilar nerves, not quite as thick, diverge near the base of the leaf to the point of the lower lobe. The medial nerve branches from the middle; the veinlets seem to be perpendicular to the nerves but obsolete; the ultimate reticulation has pretty large, hexagonal, polygonal or square meshes, with their veinlets as distinct as those of the areas.—This leaf, for its general outline and nervation, is similar to *D. Dechenii* Web., Paleont. ii, p. 193, pl. 21, fig. 10, from the Tertiary lignitic formation of the Under Rhein., differing by proportionally greater length, obtuse lobes, round base, &c.

42. *Acer obtusilobum?* Ung.—An entire, well preserved leaf, membranaceous, triangular in outline, obtusely palmately five-lobed; basilar lobes broad and undulate, superior divisions short and obtuse.—This species might be considered as identical with that published under this name by Unger (Chloris, p. 134, pl. xliii, fig. 12 and 13), if it was not for a marked difference in the nervation. In Unger's species, the upper lateral primary nerve divide upward near the middle, and the branch ascends parallel to the medial nerve, uniting with the secondary nerves above. This kind of nervation, resembling that of a *Menispermum,* is observable in the follow-

ing species. In this leaf the nervation is craspedrome and camptodrome as in other species of *Acer*.

43. *Acerites menispermifolius*, sp. nov.—A small leaf, triangular in outline, five lobate, with short obtuse irregular lobes. Primary nerves in five from the base, the upper lateral one branching in the middle with one of the divisions ascending upward. The base of the leaf is nearly truncate. It may belong to the former species or to a true *Menispermum*. Another small leaf, also triangular, with a cuneate base, three obtuse equal short lobes, the middle one abruptly short pointed and a similar kind of nervation, may be a variety of the same species.

44. *Negundoides acutifolia*, sp. nov.—Two thin leaves, which appear to have been attached on a same common petiole, and therefore to belong to a compound leaf. The upper leaflet is smaller, ovate, lanceolate-pointed, entire; the lower one is enlarged on one side, dentate-lobed, like an *Acer*, and with the same kind of nervation, while the other side is narrow, entire, with merely secondary nerves as in the upper leaf. Prof. Unger in his Chloris has figured and described, under the name of *Acer*, two lanceolate leaves, slightly dentate, which are opposite on a common rachis, and belong, therefore, to a compound leaf resembling a *Negundo*. They have some affinity with ours.

45. *Paliurus membranaceus*, sp. nov.—Leaf exactly oval-obtuse, of a thick membranaceous substance and polished surface; palmately three-nerved from the base; lateral nerves ascending to above $\frac{2}{3}$ of the leaf, branching outward and united to the thicker medial nerve by perpendicular veinlets.

46. *Rhamnus tenax*, sp. nov.—A fine leaf represented by two fragments on the same specimen. It is lanceolate or slightly ovate-lanceolate, gradually tapering to a point and narrowed to a short petiole still attached to a branch. The secondary nerves, 14–15 pairs, ascend in an acute angle of 30°, and curve along the borders; the veinlets are obsolete, nearly continuous and perpendicular to the veins.

47. *Phyllites Rhoifolius*, sp. nov.—Two fragments of an oblong subcoriaceous thick leaf, with a broad medial nerve and secondary nerves alternate, emerging in an open angle, arched upward, curving near the borders where they unite. The borders of these leaves appear marked with distant irregular denticulations like those of our *Rhus cotinoides*, which this species resembles, also, by the nervation. The base and top of these leaves are destroyed.

48. *Juglans Debeyana* (*Populus?*) Heer.—The general form of the leaves of this species, represented by many specimens, is broad, oval and slightly obtuse; sometimes oval-lanceolate,

with a blunt point; borders always entire. Some of the leaves are equal on both sides, and have a pretty long petiole; some others are broader on one side or inequilateral, often curved to one side from a short petiole, thus showing the form of leaflets of a compound leaf. The nervation is exactly that of a *Juglans;* I consider, therefore, these leaves as belonging to this genus.

49. *Prunus Parlatorii* (*Andromeda?*) Heer.—Among the numerous specimens of this species, one of them has distinctly preserved its nervation, which is like that of a *Prunus* or of an *Amygdalus.* As the leaves are all entire, coriaceous, all lanceolate pointed, attenuated to the petiole which, like the medial nerve, is striated lengthwise, they have the essential characters of the *Prunus.* The following fruit tends to support this assertion.

50. *Prunus Cretaceus,* sp. nov.—A broadly ovate-pointed nutlet, similar in every point to a large kernel of a *Prunus,* or to a very small almond. The surface is smooth; the truncate base is notched by a groove ascending to above the middle of the nut. Another nutlet of the same kind seems to have its whole thickness imbedded in the stone showing merely its back; though it is figured, its relation is not ascertained.

51. *Phyllites Vannonæ* Heer.—The specimen does not differ from the figure and description of this species in Phyllites du Nebraska. The leaf is ovate, taper-pointed, attenuated to the base, entire, and slightly more enlarged on one side, at least near the base. Medial nerve narrow; secondary nerves obsolete.

52. *Phyllites umbonatus,* sp. nov.—A leaf perhaps deformed by compression, for the medial thick nerve is split in two from the top to the middle of the leaf, which is thus deeply cut or emarginate. The leaf about orbicular in outline is truncate at the base, irregularly lobate on the borders, with three or four pairs of secondary veins, the upper ones curved upward, those of the middle nearly at a right angle with the medial nerve, and the inferior turned downward.

53. *Phyllites amorphus,* sp. nov.—Two fragments of a coriaceous obovate entire leaf, narrowed toward the petiole; medial nerve deep and narrow; secondary nerves either horizontal, or turned downward, or ascending in an acute angle, branching also in various abnormal ways.

To complete the list of fossil plants known at this time from the Cretaceous formation of America, we have only to add to the above enumeration the plants collected by Dr. F. V. Hayden, of which a short account is given by Prof. Heer in Proceed. of Nat. Sciences of Phil., 1858, p. 265; those of Profs. Marcou

and Capellini, described and figured by Prof. Heer in "Phyllites Crétacés du Nebraska;" and those sent to me, still later, by Dr. John LeConte, which are figured and described in Appendix to Tertiary fossil plants of the Mississippi State, in Trans. Amer. Phil. Soc., vol. xiii, p. 430, pl. 23. Considering the species we find:

1. *Lyriodendron Meekii* Heer, Proc. Acad. of Nat. Sciences.—A small leaf with short round lobes not found among our specimens.
2. *Sapotacites Haydenii* Heer, loc. cit.—A leaf of unknown affinity.
3. *Laurus primigenia* Göpp. in Heer, loc. cit.—Not seen among our specimens and evidently different of *L. Nebrascensis*.
4. *Leguminosites Marcouanus* Heer, loc. cit.—Unknown to me. The author compares it to a *Cæsalpinia*.
5. *Populus leuce* Ung. in Heer, loc. cit.—Not satisfactorily recognized.
6. *Populus cyclophylla* Heer, loc. cit.—Probably the same species as the one of ours to which the specific name is preserved.
7. *Phyllites obtusilobatus* Heer, loc. cit.—An imperfect fragment, perhaps referable to *Lyriodendron Meekii*.
8. *Phyllites obcordatus* Heer, loc. cit.—Of unknown affinity.
9. *Populus litigiosa* Heer, Phyll. du Nebraska.—Fragment. Not seen in our specimens.
10. *Populus? Debeyana* Heer, loc. cit.—Described above as *Juglans Debeyana*.
11. *Salix nervillosa* Heer, loc. cit.—Not seen among our specimens.
12. *Betulites denticulata* Heer, loc. cit.—Not seen.
13. *Ficus primordialis* Heer, loc. cit.—Not seen.
14. *Platanus? Newberriana* Heer, loc. cit.—Described above.
15. *Proteoides Grevilliæformis* Heer, loc. cit.—Described above.
16. *Proteoides acuta* Heer, loc. cit.—Described above.
17. *Proteoides daphnogenoides* Heer, loc. cit., and Trans. Am. Phil. Soc., vol. xiii, pl. 23, fig. 8.
18. *Aristolochites dentata* Heer, loc. cit.—Not seen.
19. *Andromeda Parlatorii* Heer, loc. cit.—Considered in this paper as a *Prunus*.
20. *Diospyros primæva* Heer, loc. cit.—Not seen.
21. *Cissites insignis* Heer, loc. cit.—Not seen.
22. *Magnolia alternans* Heer, loc. cit.—Described above.
23. *Magnolia Capellini* Heer, loc. cit.—Not seen.
24. *Populites coriaceus* Lesq., Proc. Am. Phil. Soc., vol. xiii, p. 430, pl. xxiii.
25. *Phyllites Betulæfolius* Lesq., loc. cit.
26. *Persæa Nebrascensis* Lesq., loc. cit.—Considered as a *Laurus*.
27. *Sassafras LeConteanus* Lesq., loc. cit.
28. *Cinnamomum Heerii* Lesq., loc. cit.

Of these species, 8 are described in this paper and therefore we have to add to our number twenty, making a total of 73 species, or rather forms of leaves, from our Cretaceous.*

* It is well understood that when the word *species* is used in an examination of fossil plants, it is not taken in its precise sense. For indeed no *species* can be es-

Considering that we know scarcely anything of the Cretaceous flora of other countries, this number, though small, is indeed a valuable contribution to science. The scarcity of Cretaceous remains of plants explains perhaps the great number of species established from the Nebraska specimens. In his Phyllites, Prof. Heer remarks that between the Cretaceous plants of Nebraska and those of Europe, there are no identical species. The celebrated palæontologist sent to Dr. Debey of Aix la Chapelle, who has there discovered quite a Cretaceou flora, the drawings of the species collected by Messrs. Marcou and Capellini, inquiring about their relation to what had been discovered in Belgium. The answer was, that none of the species were identical; that even the genera were different. Of course we can not conclude from this that there is not any existing relation between the Cretaceous flora of our continent and that of Europe, because the materials which may serve as point of comparison may not have yet been discovered. Prof. Heer already finds some generic affinity between a Cretaceous flora of Moravia and ours, both containing *Magnolias* and *Ficus;* and from the remarks on the species described from Dr. Hayden's specimens, it is seen that the same generic affinity is observable with the Cretaceous flora of the Hartz mountains by one *Credneria* and one *Pterophyllum.* Nevertheless, the more we know of the floras of the geological ages of America, the more we recognize in them peculiar types which in their grouping constitute what may be called an *American facies*, which by successive transitions has passed to our present flora and assigned to it its general character. Is it not remarkable, for example, that our Cretaceous fossil plants should have a more evident relation with our present flora than with that of any stage of the Tertiary of Europe. Some of the Cretaceous species are undistinguishable from predominant species of our time. And when we consider merely the general facies of our present arborescent vegetation, we can but recognize it in the Cretaceous. *Liquidambar*, *Populus*, *Betula*, *Fagus*, *Quercus*, *Platanus*, *Credneria* (closely allied to *Coccoloba* of which we have two species in Florida), *Laurus*, *Sassafras*, *Lyriodendron*, *Magnolia*, *Acer*, *Paliurus*, *Rhamnus*, *Juglans*, *Prunus*, &c., all genera of ours and this in seventy species discovered! Is it possible to point out a more evident characteristic affinity!

From this we may at once admit, that we do not have to look for the origin of our actual vegetation to some far distant country, and to account for its nature by peculiar and cataclys-

tablished from leaves or mere fragments of leaves. But as palæontologists have to recognize these forms described and figured, to compare them and use them for references, it is necessary to affix to them specific names and therefore to consider them as *species*.

matic transportations. Its origin is not Australian as it has been sometimes admitted, nor Asiatic, still less European; but it is born, has been cradled, and has grown up on this continent. This preservation of peculiar types, present at divers geological epochs, indicates a successive and slow development of formations without such great disturbances as are recognizable in other countries; and it proves also that the climatic conditions of our North American continent have continued about the same as they are now from the Cretaceous through the Tertiary. No species found in these formations of ours indicates a warmer temperature than that of the Southern States.

We know very little yet of the vegetation of our Tertiary formations, and it is impossible to attempt now a comparison of the floras of the Tertiary and of the Cretaceous in America. Nevertheless, from the species already published, even from those of the Tertiary of Nebraska, obtained by Dr. Hayden and Dr. LeConte, the generic affinity is striking and therefore the general American facies is equally represented in both.

Vegetable remains are the records of the natural phenomena which have governed the surface of our earth at different epochs. Nowhere else can the successive development of a long series of vegetable cycles, without cataclysmatic interruptions, be followed as well as in America. When, then, the fossil plants of our country have been thoroughly studied, they will unfold to us the history of nature's proceedings during the geological times. Questions of a high order are therefore intimately allied to the study of those remains of fossil plants so little valued among us even now.

Columbus, March 19, 1868.

ART. XIII.—*Recent Eruption of Mauna Loa and Kilauea, Hawaii.*

[The following are extracts from some of the accounts of the recent great eruption on Hawaii which have reached us. The reader will be aided in understanding the geography of the island by referring to a map in volume xxvii of this Journal, p. 412, (or that in the writer's Manual of Geology, p. 696.) The district of *Kona* is on the west side of the island, between *c* and *f* nearly; that of *Kau*, on the southern, from *f* to a line running S.S.E. from Kilauea; that of *Puna*, east of Kau, and south of Hilo. *Kahuku* lies almost in a direct line between the summit of M. Loa and the south cape, 12 or 15 miles north of the cape, and this line was the course of the principal fissures

and flows of lava. The *new island* is situated just south of the south cape. *Waiohinu* is about 8 miles east of Kahuku (north of *k* on the map), 5 or 6 miles from the sea at *Kaalualu*. *Honuapo* is at *i*; *Punaluu*, 4 or 5 miles farther east on the coast. *Hiilea* lies 3 or 4 miles east of north of Punaluu. From Punuluu the road to Kilauea leaves the coast and passes through *Makaka*, *Keaiva* (about 26 m. from Kilauea), *Kapapala* (18 or 20 m. from K. and 3000 feet above the sea). Mr. F. S. Lyman lived near Keaiva. The mud-eruption of which he speaks in his letter below was a little northwest of a line between Keaiva and Kapapala; and the lava eruptions near Hiilea are west of south of the mud-eruptions; both appear to have been connected with the discharge of Kilauea, while the eruptions near Kahuku and in the line east of north from this place may have come direct from the central crater of the mountain.

The letter of Mr. Lyman to his brother, David B. Lyman of Chicago, was received by us through Prof. Lyman of New Haven. For other communications we are indebted to Prof. Alexander, of Honolulu, and the Honolulu papers.—J. D. DANA.]

1. *Letter from Rev.* TITUS COAN, (*to* J. D. DANA,) *dated Hilo, Hawaii, April* 7, 1868.

History and tradition record no such commotion on Hawaii as we have just experienced. On the 27th of March, slight and repeated shocks of earthquake were felt in Kau, the southern district of this island. On the 28th these shocks became more frequent and more energetic, extending to Hilo, Kona, and probably to all parts of the island. On this day the inhabitants of Hilo saw steam and fire shooting up from several points on the summit, and down the S. E. slope of Mauna Loa. During the same day, all Kau was startled by heavy explosions and roarings, as of a tempest, from the mountain. The mountain was rent, apparently, from near the summit crater, Mokuaweoweo, half way down its southern slope, and jets of steam and smoke went up from many points, while four distinct streams of lavas flowed out from separate fissures, and rushed down the mountain. One of these streams flowed nearly due south half way down the mountain toward Kahuku. At the same time a terrible earthquake shook down the large stone church at Kahuku, and also all the stone dwelling houses in that place, including the houses of some foreign gentlemen, who are grazing cattle at the foot of the mountain.

A letter just received from Rev. T. D. Paris, of Kealakekua, South Kona, and dated March 29th, contains some facts con-

cerning that side of the island, which I take the liberty to transcribe.

"For the last 36 hours, our house and all about us have been trembling, shaking and heaving, as if the very foundations were giving way. For ten hours there was a succession of shakes at intervals of from two to five minutes—vibrations, roaring and hissing, continuing most of the time, from one shock to another.

"Yesterday, during the heaving of the earth, four avalanches fell from the Kaanaloa precipice into the bay.

"Friday morning (27th), between 5 and 6 o'clock, we discovered the great mountain to be on fire, with immense columns and pillars of smoke; but as yet we are ignorant as to the course of the stream.

"Tuesday, April 1st. The shaking of the hill still continues. We have not undressed for sleeping since Friday night."

Thus far Mr. Paris.

On Saturday, the 28th, the lights on the mountain disappeared on all sides, from Kona to Hilo scarcely a puff of steam was seen, and all subaërial volcanic demonstrations ceased. All eyes were looking to the hills, and all were inquiring with amazement, "What has become of the eruption?"

Meanwhile the whole island trembled and shook. Day and night the throbbing and quaking were nearly continuous. No one attempted to count the sudden jars and prolonged throes, so rapid was their succession. And even during the intervals between the quakes, the ground and all objects upon it seemed to quiver like the surface of a boiling pot. The quaking was most fearful in Kau, and anxiety marked all thoughtful minds. The truth was, all the fires of the mountain and of Kilauea were sunk in subterranean caverns and chambers, and were struggling to force their way down to the ocean. The sea of lavas must have been enormous, and it was working, underground, in numerous ducts, under a tract many miles broad. The shocks and quiverings continued with different degrees of intensity until Thursday, the 2d inst.

It was now evident that Kilauea, and the mother mountain, Loa, were acting in concert. The fires in the former had become fearfully intense, shaking down avalanches of rocks from the outer walls, cracking the earth and bursting into an extinct lateral crater, called "Little Kilauea."

At 4 p. m. on the 2d instant a shock occurred, which was absolutely terrific. All over Kau and Hilo, the earth was rent in a thousand places, opening cracks and fissures from an inch to many feet in width, throwing over stone-walls, prostrating

trees, breaking down banks and precipices, demolishing nearly all stone churches and dwellings, and filling the people with consternation. This shock lasted about three minutes, and had it continued three minutes more, with such violence, few houses would have been left standing in Hilo or Kau. Fortunately there was but one stone building in Hilo, our prison, and that fell immediately.

As this awful shock died away, the sea rose some six feet above high water mark, and all the dwellings, stores, machine shops, etc., near the shore, were in imminent peril. At the same hour all Kau experienced a much more awful convulsion.

Between Kapapala and Keaiva, about twenty-six miles from Kilauea, the earth suddenly opened, among the foot hills of the mountain, a mile or two above the road leading through Kau, and a mass of earth, stones and mud was thrown up two to three miles long, and two to three miles broad, where the opening commenced, and one half to three-quarters of a mile broad, at the terminus. This earthy eruption is said to be four to fifteen feet deep, and the disgorgement was so rapid that thirty people, and 2,500 to 2,600 bullocks and horses were crushed, and all the houses of the village buried from sight. The occurrence was on a cattle ranch belonging to Reed and Richardson, and near the sheep and goat ranch of F. S. Lyman, Esq., son of Rev. D. B. Lyman, of Hilo. At the same moment, the houses of Reed and Richardson, of Mr. Lyman, and nearly all the native houses in that district, were shaken in pieces.

Simultaneously with this, there was a submarine disgorgement of lavas into the sea,* which caused a tidal wave some 15 to 20 feet high. This was fiery red, from the enormous eruption of igneous matter which broke up under the sea for miles along the shore, sweeping away every building on the lower grounds for some fifteen miles along the coast. How many lives were lost by this influx we have not yet ascertained. I have seen forty-seven names of the killed in the earthy eruption, some six miles from the sea, and this influx of the ocean. These statistics include only the eastern portion, or less than one-fourth part of Kau. We are anxiously waiting for news from the central and western portions of the district. Before the terrible shock of the 2d, we had heard that the churches and many dwellings at Waiohinu and onward to Kahuku, were in ruins; but since that event no messenger has come from all that region to report.

It is said that the great earth eruption near Kapapala was not heated, and that there was no appearance of fire in the dis-

* So Mr. Lyman thinks.

gorgements. The whole mass was thrown out of the earth like the discharge of a cannon, with a rush of wind and an awful roar. The whole action was seen by Mr. Richardson and others on the N. E., and by Mr. Lyman and others on the S. W. side of the eruption. The premises of both these gentlemen came near being swallowed up in this upheaval.

For the last twelve days, few probably of the people of Hilo and Kau have put off their clothes for sleeping. Many have camped out in the fields, and all have been anxious to secure places of comparative safety.

We still have repeated shocks, which send us out of our houses by day and night, and our house has often jarred and quivered since I have been writing these lines. But the shocks are less frequent and severe from day to day.

The sympathy between Kilauea and Mauna Loa has been distinctly marked during this eruption. Like the divisions of a grand army, all the Plutonic forces of our island have seemed to act in concert; the fires in the mountain and in Kilauea rising and falling together, and the great subterranean movements, and the rush into the sea, being simultaneous. *The fires of Kilauea have been drawn off and the crater has sunk down several hundred feet, as in the eruption of* 1840.

For four or five weeks previous to this eruption, we had heavy and continuous rains, and a vast amount of water must have gone down into the earth, filling the subterranean streams and reservoirs, and causing them to overflow. The descent of such quantities of water to the rising columns of lava, and the rapid generation of steam, may have hastened and intensified the catastrophe.

We give you the facts so far as we are able to do so, leaving the philosophical reasonings and conclusions to our scientific friends.

April 10. Last evening news came in from Waiohinu and Kahuku, that all that region was in ruins. The terrors were awful. Not an undamaged house left. 67 lives lost by the influx of the sea, and no shore village standing. The lavas have broken ground in Kau and are flowing to the sea. Our shocks still continue at intervals.

2. *Extracts from a letter from Mr.* FREDERICK S. LYMAN, *dated Hilo, April* 10*th*, 1868, *addressed to* D. B. LYMAN, Esq., *of Chicago.*

. . . . In my last letter from Kau I left off writing Tuesday evening, the 31st ult. That night from about ten till two the shaking was almost incessant; it then subsided. Wednesday morning about sunrise there was a hard shock, and

again at 5 P. M. there was a severe and protracted earthquake, with considerable swaying to and fro of the earth. Nearly all that night the shaking was very severe with frequent shocks and a rumbling sound from the south. * * * *

Soon after four o'clock P. M. on Thursday we experienced a most fearful earthquake. First the earth swayed to and fro from north to south, then from east to west, then round and round, up and down, and finally in every imaginable direction, for several minutes, everything crashing around, and the trees thrashing as if torn by a hurricane, and there was a sound as of a mighty rushing wind. It was impossible to stand; we had to sit on the ground, bracing with hands and feet to keep from being rolled over. While this agitation was at its height we saw bursting out, from the top of the bluff, about a mile and a half north of us, what we supposed to be an immense torrent of molten lava, which rushed across the plain below, apparently bursting up from the ground as it went, throwing rocks high in the air, and swallowing everything in its way;—trees, houses, cattle, horses, goats and men, all overwhelmed in an instant. This devouring current passed over a distance of about three miles in as many minutes, and then ceased. Some one called our attention to the ocean, and as soon as the severity of the earthquake had sufficiently subsided, we ran to a place where we could see the coast. All along the shore from directly below our place to Punaluu, a distance of three or four miles, the sea was boiling and foaming furiously. The waves covered the shore, and the water was red for at least the eighth of a mile from the land.

With our children and our native servants we went immediately to Nahala's hill, a short distance west of our house. From the hill-top we could overlook the country. At Hiilea, a short distance farther west, a small stream of black smoking lava was oozing from the earth; and outside of the harbor at Punaluu a long black column of lava pushed itself slowly into the ocean, and finally disappeared under the waves. We stood, expecting every moment to be swallowed up by the earth, for there were frequent earthquake shocks, the ground was opening with numerous fissures, every rock and crag that could fall had fallen, and there was a continual roar as if the molten lava was surging and rushing directly under our feet. * * *

The villages on the shore were swept away by the great wave that rushed upon the land immediately after the earthquake. The eruption of earth destroyed thirty-one lives, but the waves swallowed a greater number. * * *

Mr. Richardson returned to Kau on Monday, reaching his farm at Kapapala, Tuesday noon; but as the earthquakes were

very frequent and very severe, he remained only two or three hours. While he was camping, that night, on the great sand-plain southwest of Kilauea, the clouds were lifted from the mountain, and he saw a great river of lava pouring down its southern slope in the vicinity of Waiohinu or Kahuku, and entering the sea near Kaalualu bay. At Kilauea he could see neither fire nor smoke in the crater. Last evening (Thursday), with two other white men and several natives, he sailed on the sloop Live Yankee, bound for the coast of Kau, hoping to be able to rescue all who are still endangered by the volcano.

Since last Saturday evening, the earthquake shocks at this place (Hilo) have been infrequent and very slight, but the mountain still smokes furiously. I should have mentioned the fact that in the morning of the day after the great earthquake we could see that there had been small eruptions of earth in the margin of the forest all along the side of the mountain, from the high land above Mr. Richardson's house to the hill behind our house, a distance of four miles or more. The tract covered by the great eruption was nearly a mile wide and three miles long, forming a bank of moist, clayey soil fifteen or twenty feet high. It looks precisely like a great bank of red and brown clinkers (the *aa* of the natives). A stream of water is now running through it, and far below toward the ocean.

In the earlier letter of Mr. F. S. Lyman, (dated Kau, March 31st,) addressed to D. B. Lyman of Chicago, he writes as follows:

Since we last wrote you we still live in fear and trembling. You may have already heard that an eruption broke out on Mauna Loa, a little to the southwest of the summit, about six o'clock last Friday morning, the 27th inst., without any forewarning. The fire burst up out of the ground, throwing a spray of red lava high in the air; and then a great column of smoke rose straight up thousands of feet and arched over to the east; in a few minutes a new jet was thrown up a little southeast of the first, with its column of smoke. This was soon followed by another, and then by a fourth; and soon the red lava began running down the sides of the mountain in four streams, in a southerly and easterly direction. About seven o'clock we began to hear a roaring sound which grew louder and louder until the air seemed to tremble with the incessant roar of the volcano, but finally it subsided and ceased entirely about eight o'clock. But before that time the clouds had shut down on the mountain so that we could see nothing more then. About noon we began to feel slight earthquakes, and during the night they were very frequent, some of the time, every minute or two; though very slight, they were sufficient to prevent sleep, for

almost every jar would rattle the doors and windows. After sunrise Saturday morning (28th) the shocks began to be harder, coming often two together; they increased in violence until between one and two o'clock in the afternoon, when there occurred the hardest of all, with three shocks, which lasted about a minute; the swaying motion was so great that it was hard to stand up. The rest of the day the shocks were not very severe. Saturday morning we could see the smoke, and the flow had apparently gone about ten miles due south from the source, but during the day and part of the night it was covered with clouds.

Saturday night the shocks were very frequent and quite severe; and the house made such a noise and commotion at every shock that we all moved to our native house, or none of us would have slept any. The next morning, Sunday, the line of smoke had advanced about 15 miles since the morning previous, and seemed to be directly in the line toward Capt. Brown's house in Kahuku; but, what seems singular, no light had thus far been visible except an occasional show of it at the source. Sunday the shakes were less frequent, with some hard ones until about 2 P. M. when they nearly ceased; occasional and slight all that night, and Monday also, and Monday night; but to-day (Tuesday, the 31st), about ten A. M. there was quite a severe shaking, and at five P. M. a harder one.

3. *Extracts from a letter from* H. M. WHITNEY, *editor of the Honolulu Advertiser, dated Kealakekua, April* 13*th, on the eruptions near Kehuku.* (From the Advertiser.)

On ascending the ridge just west of and opposite the Mamalu Pali (precipice) of Kahuku, and which was separated from us by a valley about one-eighth of a mile wide, the whole scene opened before us in one grand panorama. The valley itself was floored over with a pavement of fresh *pahoehoe* lava (solid lava stream having a nearly smooth, though often rippled and wavy surface) from ten to twenty feet deep, which appears to have been the first thrown out, and came from a crater about ten miles up the mountain, which burst out on Tuesday morning, April 7th. This crater and stream had ceased flowing, and the lava was rapidly cooling, so that we ventured to stand on it, though at the risk of burning our boots and being choked by the sulphurous gases. On Tuesday afternoon, at 5 o'clock, a new crater, several miles lower down, and about two miles directly back of Captain Brown's residence, burst out with a heavy roar and a frightful crash. The lava stream commenced flowing rapidly down the beautiful plateau, toward and around the farm-house, and the inmates had barely time to escape with

what clothes they had on, before the houses were all surrounded and enclosed with streams of fiery *aa* lava varying from five to fifty feet in depth. Fortunately all the inmates escaped safely to Waiohinu: but how narrow the escape was, and how rapid the stream flowed, may be inferred from the fact that the path by which they escaped was covered with lava within *ten minutes* after they passed over it.

The new crater, when visited by Mr. Swain, was at least one and a half miles in extent, nearly circular, but constantly enlarging its area, by engulphing its sides. While the above gentleman was looking at it, a tract of at least five acres in extent tumbled in and was swallowed up like food for the devouring element. The enlargement is going on mainly on the lower side, toward the farm-house, and it is thought that its diameter is already about two miles. Four huge jets or fountains were continually being thrown up out of this great crater, ever varying in size and height, sometimes apparently all joining together and making one continuous spouting a mile and a half long. From the lower side of the crater a stream of liquid, rolling, boiling lava poured out and ran down the plateau, then down the side of the pali (following the track of the government road), then along the foot of the pali or precipice five miles to the sea.

This was the scene that opened before us as we ascended the ridge on Friday (10th). At the left were these four grand fountains playing with terrific fury, throwing blood-red lava and huge stones, some as large as a house, to a height varying from 500 to 1,000 feet. The grandeur of this scene, ever changing like a moving panorama, no one who has not seen it can realize.

Then there was the rapid, rolling stream, rushing and tumbling like a swollen river, down the hill, over the precipice and down the valley to the sea, surging and roaring like a cataract, with a fury perfectly indescribable. This *river of fire* varied from 200 to 800 feet in width, and when it is known that the descent was 2,000 feet in five miles, the statement that it ran at the rate of *ten to twenty-five miles an hour* will not be doubted.*

We waited till night, when the scene was a hundred fold more grand and vivid. The crimson red of the lava now doubly bright, the lurid glare of the red smoke-clouds that overhung the whole, the roaring of the rushing stream, the noise of the tumbling rocks thrown out of the crater, the flashes of electric lightning, and the sharp quick claps of thunder—altogether made the scene surpassingly grand.

* Some corrections are here introduced from Mr. Whitney's later account in the Advertiser of May 9th.—Eds.

The eruption lasted five days, it ceasing entirely in the night between the 11th and 12th.

Finding it impossible to get over to Waiohinu, either by going up the mountain or by sea, we returned to this place on Saturday, and hope to go on soon by steamer.

The number of shocks which occurred at Waiohinu from March 29th to April 10th, is estimated by Mr. Silloway to have been upward of two thousand, there having been some days between three and four hundred. The heaviest shock occurred on Thursday, April 2d, being the same that was felt so sensibly at Honolulu. This destroyed every church and nearly every dwelling in the whole district.

From 10 to 12 o'clock of that day, there had been service in the large church in Waiohinu, and it was crowded with people. Only four hours after they left the heavy shock came, the walls tumbled in, and the roof fell flat—all the work of twenty seconds. At the same instant, every man, woman and child were thrown from their feet. Horses and cattle dropped down as if dead. A man riding on horseback had his horse tumble under him so suddenly, that he found himself and horse lying flat on the ground before the thought of an earthquake entered his mind.

The earth opened all through the district, and in some places left dangerous fissures, while in others it closed up again. In one place it closed twenty feet from where it opened. These fissures make it dangerous to travel in the dark. Everywhere the roads are broken up, and it will take much money and labor to restore them to their old condition.

As the *Kona Packet* was passing the south point of the island, about three miles from the shore, a conical island, four hundred feet high, rose out of the sea, midway between the vessel and the land, emitting a column of steam and smoke. The lava river flows into the sea at this island and has extended the shore out to it one mile at least, so that it is now on the main land. The packet was so near when this island burst up, that the mud was spattered on the masts and sails of the vessel. Where the lava entered the sea, there were bluish flames emitted 10 to 20 feet high, besides steam and smoke.

The scene at the eruption was a most melancholy one to witness. There were hundreds of fine cattle grazing around the farm-houses, when the lava streams surrounded them and hemmed them in. The poor animals seemed aware of the danger, but saw no way to escape. The fiery lava drew nearer and nearer, till the heat made them restless, but they would not run. They bravely looked the foe in the face, stood firm till it reached them, then fell in the stream—a sudden cloud of smoke

followed, and not a sign remained. Thus one after another fell till over two hundred were consumed.

An incident which ought not to be omitted is the shower of ashes which preceded the eruption. During Monday night (the 6th), prior to the eruption, the ground throughout the district was covered with a coating of fine sand and light pumice stone, of a light yellowish color. Where this shower of sand and pumice stone came from is as yet unknown, but probably from some vent hole near the summit crater.

The tidal wave was much greater than before stated. It rolled in over the tops of the cocoanut trees, probably sixty feet high, and drove the floating rubbish, timber, etc., inland a distance of a quarter of a mile in some places, taking out to sea when it returned, houses, men, women, and almost everything movable. The villages Punaluu, Ninole, Kawaa and Honuapo were utterly annihilated.

4. *Letter of* Dr. Wm. Hillebrand, *on the Crater of Kilauea and the eruptions southwest*, published in the Hawaiian Gazette.

Dr. William Hillebrand visited the crater of Kilauea and the scene of the mud flow, and has published a very interesting report of his observations, from which the following extracts are taken.

The ground around the crater of Kilauea, particularly on the eastern and western sides, is rent by the great number of fissures, one near the Puna road more than twelve feet wide and very deep. Others of lesser size run parallel to and cross the Kau road so as to render travel on it very dangerous. The look-out house is detached from the mainland by a very deep crevasse, and stands now on an isolated, overhanging rock, which, at the next severe concussion, must tumble into the pit below. Many smaller fissures are hidden by grass and bushes, forming so many traps for the unwary. The Volcano House, however, has not suffered, nor is the ground surrounding it broken in the least. From the walls of Kilauea large masses of rock have been detached and thrown down. On the west and northwest side, where the fire had been most active before the great earthquake of April 2, the falling masses probably have been at once melted by the lava and carried off by its stream, for the walls there remain as perpendicular as they were before, but that this portion of the wall has lost portions of its mass, is shown too evidently by the deep crevices along the western edge just spoken of, and the partial detachment in many places of large prisms of rock. But it is on the east and northeast wall particularly, that the character of the crater has undergone a change. Along the descent on the second ledge, large

masses of rock, many more than one hundred tons in weight, obstruct the path and form abutments to the stone pillars—small buttress hills, similar to those observed in front of the high basaltic wall in Koolau, Oahu. So also in the deep crater itself, the eastern wall has lost much of its perpendicular dip, and has become shelving in part.

The crater itself was entirely devoid of liquid lava; no incandescence anywhere; pitchy darkness hovered over the abyss the first night. I say the first night, because during the second night of our stay, between twelve and one A. M., detonations were heard again, and light reappeared for a short time in the South Lake. White vapors of steam issued from the floor in a hundred places, but of those stifling, sulphurous and acid gases formerly so overpowering in the neighborhood of the lakes and ovens, only the faintest trace was perceived. The heat was nowhere so great over the bottom that we could not keep our footing for a minute or more, although in many places it would forbid the touch of the bare hand. The great South Lake is transformed into a vast pit, more than five hundred feet deep, the solid eastern wall projecting far over the hollow below, while the remaining sides are falling off with a sharp inclination, and consist of a confused mass of sharp *aa*. More than two-thirds of the old floor of Kilauea has caved in, and sunk from one hundred to three hundred feet below the level of the remaining floor. The depression embraces the whole western half, and infringes in a semicircular line on a considerable portion of the other half. It is greatest in the northern and rather gradual and gentle in its southern portion. Entering upon the depressed floor from the southern lake, it was some time before we became fully aware of its existence. It was only on our return from the northwest corner, where it is deepest, that there presented itself, through the mist in which we were enveloped, a high wall of three hundred feet of grotesque and fantastic outlines. At first we were quite bewildered, fancying that we beheld the great outer wall of the crater. On nearer approach, we soon satisfied ourselves that this singular wall represented the line of demarcation of a great depression in the floor of the crater, a fact that surprised us the more, as a bird's eye view from above had altogether failed to apprise us of its existence.

As we had been informed that the principal activity of the crater before the great earthquake had been in the northwest corner, we proceeded in that direction on leaving the south lake. Having arrived at about the middle of the depression, a considerable rise in the ground presented itself on our left—to the west. Having ascended this, we found ourselves at the

brink of a fearful chasm, which fell off on our side with a beetling wall to the depth of several hundred feet, and extended about half a mile from north to south. Very hot air rose from it. Around it, toward its northern extremity, the lava is thrown up into an indescribable confusion; pile upon pile of *aa*, gorge and ridge by turns.

The caving in of the floor seemed to be still in progression, for twice during our exploration of the crater our nerves were disturbed by a prolonged heavy rumbling and rattling noise, as from a distant platoon fire of musketry, coming from the northwest corner. * * * * *

Thus far as to what we have seen. Now allow me to relate what I learned from Kaina, who has resided near the volcano without interruption for the last five months, and whose strong nerves sustained him during the fearful catastrophe introduced by the earthquake of April 2. He and the Chinaman who keeps the house, were the only persons who remained at Kilauea. He says for two months preceding the first shock, viz: from January 20 to March 29—the crater had been unusually active, eight lakes being in constant ebullition and frequently overflowing. During all this time (the date of its first appearance could not be ascertained exactly) there was in the northwest corner a "blow-hole," from which, at regular intervals of a minute or less, with a roaring noise, large masses of vapor were thrown off, as from a steam engine. This ceased about the 17th of March. At the same time the activity of the lakes became greatly increased, and Kaina anticipated mischief. March 27 the first shock was perceived. Two days later, Mr. Fornander found the bottom of the crater overflowed with fresh lava and incandescent.

Thursday, April 2, at a few minutes past four P. M., the great earthquake occurred, which caused the ground around Kilauea to rock like a ship at sea. At that moment there commenced fearful detonations in the crater; large quantities of lava were thrown up to a great height; portions of the wall tumbled in. This extraordinary commotion, accompanied with unearthly noise and ceaseless swaying of the ground, continued from that day till Sunday night, April 5, but from the first the fire began to recede. On Thursday night it was already confined to the regular lakes; on Saturday night it only remained in the great south lake, and on Sunday night there was none at all—Pele had left Kilauea. The noises now became weaker and were separated by longer intervals. By Tuesday, April 7, quiet reigned in Kilauea. On that afternoon the lava burst out at a distance of forty miles southwest, in Kahuku.

In Kapapala we were told that fire had been seen several nights in a southeast direction, and that natives had reported flowing lava there. We rode over on the morning of April 20. At a distance of five miles from Mr. Reed's dwelling, where the Puna road turns off from the Kilauea road, heavy clouds of white vapor were seen to issue from the bush, which sparsely covered the *pahoehoe* south of the road. Half an hour's ride brought us up to the place, but we were obliged to leave our horses some distance before reaching the spot, on account of fissures. After having crossed a number of them, heading for the heaviest cloud of vapor, we at last came to a deep crevasse in the *pahoehoe* at least twenty-four feet in width, no bottom visible. It narrowed and widened out in places, but nowhere was less than eight feet wide. Its length was estimated at four hundred feet. Parallel with this great crevasse, constituting a belt about six hundred feet in width, were a number of smaller ones on each side, diminishing in size with distance from it, from six feet to a few inches. From the larger openings in the former, heavy white columns of hot steam issued, which had a decidedly alkaline smell. Smaller jets of vapor, to the number of thirty, rose from the smaller fissures. We could not discover fire in any place, but it is very probable that during dark nights the reflection of the underlying lava should be thrown up; for as the steam did not seem to contain combustible material, it is unlikely that the light seen should have been produced by it. The mean direction of all the fissures was northeast nine degrees north, southwest nine degrees south, or nearly the direction of a line connecting Kilauea with Waiohinu and Kahuku. The distance of these fissures from Kilauea is thirteen miles.

Since the earthquake of April 2 reached its greatest intensity in this district, so as even to rend in twain the framework of a mountain and hurl down on the plain a portion of its flank, it is necessary to give a short description of the country in order to insure a proper understanding of the disturbance. The locality in question is that comprised between the ranch station of Messrs. Reed & Richardson, on the east, and Mr. F. S. Lyman, on the west, a distance of five miles. The government road connecting these two places runs through a fine grassy plain, which has a very gentle fall toward the sea, its elevation being about two thousand feet. Into this plain project from the slope of Mauna Loa three parallel hills, or spurs, each about one mile in length, and from eight hundred to one thousand feet in height. They include two broad valleys between them. The upper portions of these valleys rise with a steep inclination toward a ridge which runs at right angles with

the spurs, and is covered with a dense pulu forest, which extends far up the gentle slope of the dome of Mauna Loa. In the second one of these valleys—that next to Mr. Lyman's—the so-called *mud flow* took place; but very extensive landslides, confined simply to the loose earth and conglomerate, also occurred in the other valleys.

The ground around Reed & Richardson's station is intersected by numerous small cracks and fissures, running in every direction. * * * * The magnitude of the force was such as to shake off the face of the pali, burying in a minute thirty-one human beings, many hundred head of cattle, entire flocks of goats, and ending four miles from its beginning in a mighty river of mud. Before reaching this mud flow, from Reed's house, we passed two considerable streams of muddy water, of a reddish yellow color, emitting a strong odor of clay, such as may be perceived in potteries. Both streams have their origin in the landslide of the first valley. When we passed them again, two days later, they had nearly disappeared. They evidently owed their origin to the drainage of the fallen mass. The mud flow is met with three miles from Reed's. It projects itself from the spurs of the hills two miles down on the plain; begins at once with a thickness of six feet, which, toward the middle, where it forms a small hill, rises to thirty feet, averages about three-fourths of a mile in width, and contracts toward its end. From this end a long cue of boulders bears witness to the violent action of a torrent which shot out of the mud after it was deposited, and which has since perpetuated itself in a stream of some size, quite muddy, and emitting the above mentioned pottery odor, when we saw it first, on April 20, but perfectly clear and inodorous when we passed it three days later. A little higher up, a koa grove gives still stronger evidence to the strength of the propelling force. The trees first seized are snapped off and prostrate, yet the mud in that place is only a few feet deep. The mass itself is nothing but the loose red soil of the mountain side, with a good sprinkling of round boulders, with here and there stumps of trees, ferns, happuu and amaumau and entire lehua trunks. Near the lower end, a vigorous, healthy taro plant stood erect in the mud, as if it had been planted there. From its sides protruded portions of the bodies of many cattle and goats, overwhelmed in their flight; a gain of one second in time might have saved them. The surface of the mud in this lower course was rather smooth, as if it had been forced down by the agency of water, and it was still so soft that the feet sank deep into it. After we had flanked it for some distance along the side of the hill, the mud became solid enough to bear our weight, and we walked upon it to the head of the pali. The surface gradually

became more rough; the boulders increased, and detached portions of earth and stone were scattered beyond its borders, which also flattened out gradually. The ascent soon became steep, and here, on a short spur, just in the middle of the mud, stands a native house on an island of grass and taro, flanked by two trees. A poor woman who happened to be in it at the time of the outbreak escaped the awful fate which doomed the remaining members of her family, and was removed from her perilous situation a few days after, when the crust had become solid enough to bear a man's weight.

As we went on, the mass became more rough and hard, tree trunks and boulders increased, even angular rocks appeared, until, at last, the mud ceased entirely, and gave place to a sea of huge rocks, all angular and exhibiting fresh fractures, large trunks of trees crushed between and under them, and streamlets of fresh, clear water, meandering between them. This continued for the last three hundred feet of rise, and ended in a perpendicular wall of solid rock some twenty feet high, after having climbed which, we reposed under the refreshing shade of tall fern trees, for we had entered at once the great pulu forest. Seated on the trunk of a prostrate tree, we could survey the whole field of devastation we had just traversed. Immediately at our feet the rocky framework of the pali was torn up, and its contents turned topsy-turvy in dire confusion. The rocky wall we had just climbed continued itself, until it reached the sides of the two flanking hills. A perpendicular cut in the side of the latter laid open some forty feet of red earth and conglomerate. Looking behind us, we saw that the rock we were resting on was separated from the mountain by a deep crevasse, parallel to the wall and only partly visible, as it extended under the dense trees. To our left, a clear, sparkling mountain stream leaped in a bouncing cascade over the crag, and after losing its course amid the maze of rocks, gathered itself again, flowing over the solid bed rock in a deep gorge cut in the mud. This stream had existed here before, but ere it reached half down the pali, became lost in the soil. It can easily be imagined what an amount of subsoil water must have been deposited here. Bearing this in mind, and the great depth of soil and conglomerate on this slope, as indicated by the cuts in the hill sides, there seems to be no great difficulty to explain how such enormous masses of earth, at first propelled horizontally through the air, hurled down the valley by the tremendous force which tore off the side of the mountain, should then have been seized by the propelling force of the now liberated subsoil water, and carried in a mighty stream far beyond the place where at first they were deposited.

On returning, we concluded to reach and follow the ridge of the hill flanking the stream on our left. Having arrived there, we could survey the extent of the landslides on the opposite side of the hill, which were considerable. From this place, our guide pointed out to us a human figure in the distance, moving slowly over the dreary field. It was a husband searching for the body of his wife. Our guide himself, poor fellow, mourned the loss of a wife, two little boys, and both parents. All slept their long sleep under that field of desolation. Following the crest of the hill, still covered with grass and wood, we were startled by the number of fissures and crevices intersecting it in every direction. In some places one was tempted to say, that more space was occupied by them than by the solid crust.

The direction of the solid rock wall and the *crevasse* in the forest, is northeast by north to southwest by south, nearly parallel to a line connecting Kilauea with the lava outbreak in Kahuku. The stream running from the mud-flow is likely to remain permanent, as it is a continuation of the mountain stream above, and now runs upon exposed solid bed rock.

All this destruction was the work of the great earthquake of April 2. During the five days preceding it, over one thousand shocks had been counted. On that afternoon, Mr. Harbottle, at Reed's with his men, was driving cattle across the hill toward Hilo when suddenly the earth shook violently, and a great detonation was heard behind them. Horses and cattle turned round involuntarily. The whole atmosphere before them was red and black. In a very short time this subsided—some say in one minute, others in five minutes—but a black cloud continued to hover over the scene for some time. A native, who resided less than half a mile from the scene and who had friends living on the hill, found courage enough to run to it, half an hour after the occurrence. He thrust his hand in the mud and found it cold.

From that Thursday to Sunday, the earth constantly rocked and swayed, the hills seemed to alternately approach and recede. Most people became sea-sick. Strange roaring and surging noises were heard under the ground. When the ear was applied to the earth, it would often receive a distinct impression, as if a subterranean wave struck against the earth's crust.

Another account states that Dr. Hillebrand had visited the scene of the Kahuku eruption, and had found that there was there a fissure ten to twenty feet wide, from which the lavas issued, instead of a proper crater.

5. *From the Hawaiian Gazette of April 15th, on the earthquakes of April 2d, in northern as well as other parts of Hawaii, and at the islands of Maui, Lanai, and Oahu.*

The whole island was shaken, but most violently along the western, southern and eastern flank of Mauna Loa, the damage extending from Kealakekua on the west to Hilo on the northeast. On the northern side of Hawaii, through Hamakua, Kohala and Kopa, the shocks, though frequent, were comparatively light, except the one on Thursday afternoon, but even this, though causing people to run out of their houses, did no damage to buildings. The Kohala plantation chimney and buildings were not injured. We believe that the immunity of the northern districts is owing to the mountains of Mauna Kea, Hualalai, and the Kohala Range which intervenes between them and Mauna Loa. It is believed that the Kohala Mountains are the oldest formation on the island, the volcanic fires having moved southward as successive portions of the island were thrown up.

At Kona the shock of Thursday was terrific. We are again indebted to Mr. Williamson for accurate observations of this and of the other shocks which visited that district. The vibrations are described by the residents as continuous for hours at a time, the windows and doors rattling with increasing and diminishing violence in response to the movements of the earth. A few buildings were thrown down, and the sea ebbed and flowed, leaving fish stranded on the rocks. The motion of the water was equal to about eight feet perpendicular. The people of this and the Kau district were made sick by the motion, the same as if at sea, with nausea and pains in the stomach and loss of sleep at night.

The great shock of Thursday afternoon, according to a letter from Judge Jones of Lahaina in the Hawaiian Gazette of the 8th, was felt at that place on Maui and lasted 90 seconds. It shook furniture, pictures, and walls, and the sea receded to a small extent about 5 P. M. The sea ebbed and flowed several times, the intervals between the successive flows 7 or 8 minutes. Similar shocks occurred on Lanai.

Still farther from the scene of eruption, on Oahu (150 miles from Hawaii) the earthquakes were slight. At 4 P. M. on Thursday, the first of them occurred. It was very light, and was noticed mostly by those inside of stone buildings. The majority of our people were not aware that a tremble had occurred. The motion was lateral and quick. The sea, as at Lahaina, was observed to recede and flow, but the disturbance was not great.

On Friday night we were visited again at 12:30, with another severe shock. The vibration was very perceptible, windows

and doors rattled, and many were awaked from their sleep. Another lateral shock occurred about 1 o'clock A. M., and two others before 3 o'clock.

The same shocks were noticed at Kaneohe, and one planter rushed out to look after his sugar house chimney; no harm came to it. Probably these shocks of Friday night extended over all of this Island.

[The fact that northern Hawaii was much less shaken by earthquakes than southern, the island Maui just northwest of Hawaii, feebly so, and Oahu only 150 miles distant, much more feebly, indicates that the source of the disturbance was situated directly beneath Mauna Loa, and not far (if at all) below the level of the part of the ocean's bottom lying within the Hawaiian seas, and that therefore it was eminently a local phenomenon.

The submarine rocks of the island, everywhere cavernous or somewhat loosely put together as common with volcanic accumulations, must have all cavities filled with water from the superincumbent ocean. Mauna Loa, although nearly 14,000 feet high, and 3000 square miles in area, has only one or two surface streams over more than three-fourths of this area. As the writer observes in his Geological Report on the Sandwich Islands, the larger part of the moisture that falls annually upon the cavernous lavas becomes subterranean. Owing to the numerous vertical fractures and dikes that intersect the mountain to its base (each eruption in its history having been connected with one or more deep rendings of its sides), a portion of these waters may descend vertically to great depths, while the rest follows subterranean slopes, to emerge along the shores or beneath the ocean. (Submarine outlets of fresh water streams are common about all the volcanic islands of the Pacific.) It is to be noted further that the subterranean water-courses may have in some cases considerable size from under-ground erosion.

These facts appear to afford an explanation of the recent volcanic operations. As Rev. Mr. Coan observes, the abundance of rain during the preceding months may have been a predisposing cause. The vertical channels of the mountains, filled to the brim from the rains, would have brought immense hydrostatic pressure upon the deep-seated water-chambers below. The water may thus have been forced deeply into the hot rocks ; and there suddenly converted into steam, it caused new fissures, with attendant earthquakes, and opened passages to hotter fires; thence came vaster rendings of the mountain and severer shocks, and, as a natural sequence, all that subsequently took place. J. D. D.]

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On a new mode of forming the organic sulphacids.*—Sulphacetic, isethionic, methionic, and other organic sulphacids give off sulphurous acid when heated with potash, and may therefore be regarded as derived from neutral sulphites by the substitution of half the metal by an alcoholic or acid radical. STRECKER has succeeded in effecting this substitution directly in a number of cases. Thus iodid of methyl heated to 150° C. with a solution of sulphite of sodium yields methyl-sulphite of sodium:

$$CH_3I + Na_2SO_3 = CH_3SO_3Na + NaI.$$

Under the same circumstances, bromid of ethylene and sulphite of potassium yield disulphethylenate and bromid of potassium:

$$C_2H_4Br_2 + 2K_2SO_3 = C_2H_4(SO_3K)_2 + 2KBr.$$

Trichlorhydrin and sulphite of potassium yield chlorid of potassium and the potassium salt of a new acid, which the author calls trisulphoglyceric acid.

$$C_3H_5Cl_3 + 3K_2SO_3 = C_3H_5(SO_3K) + 3KCl.$$

The chlorinated acids behave in a similar manner; thus monochloracetic acid by boiling with an alkaline sulphite, yields an alkaline chlorid and sulphacetate:

$$CH_2ClCO_2K + K_2SO_3 = CH_2\left\{\begin{matrix} CO_2K \\ SO_3K \end{matrix}\right. + KCl.$$

Under the same conditions the chlorhydrate of oxyd of ethylene yields ethionic acid:

$$\begin{matrix} CH_2OH \\ | \\ CH_2Cl \end{matrix} + K_2SO_3 = \begin{matrix} CH_2OH \\ | \\ CH_2KSO_3 \end{matrix} + KCl.$$

In these reactions either the whole or a part of the chlorine united directly to the carbon is replaced by RSO_3. By heating chloroform with sulphite of potassium, the author obtained the potassium salt of sulphodichloro-methylic acid:

$$CHCl_3 + K_2SO_3 = CHCl_2.SO_3K + KCl$$

The preceding reactions show clearly that the sulphacids contain the residue (SO_3K) directly united with the carbon by the sulphur. It is probable that the ethyl-sulphurous acids isomeric with the preceding contain the same group, but united to the carbon by means of oxygen.—*Comptes Rendus*, lxvi, 537. W. G.

2. *On the transformation of uric acid into glycocoll.*—STRECKER has found that when uric acid is heated in a sealed tube to 160°–170° C. with a concentrated solution of chlorhydric or iodhydric acids, it is transformed into glycocoll, carbonic acid and ammonia:

$$C_5H_4N_4O_3 + 5H_2O = C_2H_5NO_2 + 3CO_2 + 3NH_3.$$

The author remarks that if hippuric acid be considered as benzoic acid conjugated with glycocoll, we may in like manner consider uric

acid as a combination of glycocoll and cyanuric acid. The two acids characterizing the urinary secretions of the herbivores and the carnivores present relations which are closer than has been hitherto supposed.—*Comptes Rendus*, lxvi, 539. W. G.

3. *On an oxychlorid of silicon.*—By passing the vapor of chlorid of silicon through a porcelain tube either empty or filled with fragments of feldspar heated to a very high temperature, Friedel and Ladenberg have obtained an oxychlorid of silicon as a limpid liquid boiling at 136°–139° C. and having the formula $Si_2\Theta Cl_6$. By the action of this liquid upon absolute alcohol the hexethylic disilicate, $Si_2\Theta(\mathrm{C}_2H_5\Theta)_6$, described by Friedel and Crafts, is obtained; the analogy between this liquid and the oxychlorid is shown by the formulas,

$$\Theta\left\{\begin{matrix}Si(\mathrm{C}_2H_5\Theta)_3\\Si(\mathrm{C}_2H_5\Theta)_3\end{matrix}\right. \qquad \Theta\left\{\begin{matrix}SiCl_3\\SiCl_3\end{matrix}\right.$$

The oxyd of silicon-triethyl, $\Theta\left\{\begin{matrix}Si(\mathrm{C}_2H_5)_3\\Si(\mathrm{C}_2H_5)_3\end{matrix}\right.$, is another body in which two atoms of silicon are united by one of oxygen. As the oxyd of silicon-triethyl may be formed by heating the oxychlorid with zinc-ethyl, no reasonable doubt can be entertained as to the true constitution of the oxychlorid. The authors call attention to the analogy between the oxychlorid of silicon and perchlorinated oxyd of methyl,

$$\Theta\left\{\begin{matrix}SiCl_3\\SiCl_3\end{matrix}\right. \qquad \Theta\left\{\begin{matrix}\mathrm{C}Cl_3\\\mathrm{C}Cl_3\end{matrix}\right.$$

and suggest that the former might be called perchlorinated silicomethylic ether or oxyd of trichlorosilicon.—*Comptes Rendus*, lxvi, 539. W. G.

4. *On a universal method of reducing and saturating organic bodies with hydrogen.*—BERTHELOT has described a simple process by which, as he asserts, any organic body whatever may be transformed into a hydrocarbon containing the same quantity of carbon and the maximum of hydrogen. Alcohols, fatty acids, the aromatic bodies, the ethylene series, benzol, naphthaline and anthracene, the amids and ethylic amines, cyanogen and complex nitrogenous bodies like indigotine and albumine, all take up hydrogen. Even black matters like ulmin, coal, and wood charcoal, present no exceptions to the general rule.

The reagent which Berthelot employs is iodhydric acid in aqueous solution of density 2. The organic body is heated to 275° C. in a sealed tube for ten hours with a large excess of the acid, the excess of the latter over the calculated quantity being greater in proportion as the organic substance is poor in hydrogen. The remarkable reducing power of the iodohydric acid under the circumstances depends upon the fact that the acid is resolved at 275° into iodine and hydrogen. The special results of the investigation are as follows:

Part 1st. Series of fatty substances proper.

I. Carburets of hydrogen.

(1). Ethylenic carburets. $C_{2n}H_{2n}$. These carburets are first converted into iodohydric ethers and then into hydrurets. Thus for ethylene C_4H_4 we have

$$C_4H_4+HI=C_4H_5I \text{ and } C_4H_5I+HI=C_4H_6+I_2.$$

So that the total reaction is $C_{2n}H_{2n}+2HI=C_{2n}H_{2n+2}+I_2$.

(2.) Acetylenic carburets. $C_{2n}H_{2n-2}$. These are first converted into iodohydrates and then into hydrurets. Thus for acetylene we have

$$C_4H_2+2HI=C_4H_2(2HI) \text{ and } C_4H_2(2HI)+2HI=C_4H_6+I_2.$$

(3.) Formenes (marsh gas series) $C_{2n}H_{2n+2}$. These bodies being saturated undergo no change.

II. Alcohols.

The alcohols yield first iodids and then formenes. Thus for common alcohol and glycerine we have

$$C_4H_6O_2+2HI=C_4H_6+I_2+H_2O_2$$

$$C_6H_8O_6+6HI=C_6H_8+3I_2+3H_2O_2$$

III. Ethers. IV. Aldehyds. V. Acids.

For these substances the author quotes the results obtained in a former investigation. Various iodids, chlorids and bromids gave the corresponding hydrurets. The same was the case for the aldehyds and acetones. The monobasic fatty acids are changed into hydrurets by the substitution of hydrogen for an equal amount of oxygen. Thus acetic acid $C_4H_4O_4$ becomes C_4H_6. This reaction permits us to pass from any formene $C_{2n}H_{2n+2}$ to its superior homologue $C_{2n+2}H_{2n+4}$. Thus hydruret of ethylene $C_4H_4(H_2)$ heated with chlorine gives chlorhydric ether $C_4H_4(HCl)$, which, with cyanid of potassium, gives cyanid of ethyl, $C_4H_4(C_2HN)$. This last gives with alkalies propionic acid, $C_6H_6O_4$, which again with iodhydric acid yields hydruret of propylene C_6H_8. Cyanid of ethyl, heated directly with iodhydric acid gives hydruret of propyl directly, the reaction being

$$C_4H_4(C_2HN)+3H_2=C_4H_4(C_2H_4)+NH_3.$$

The bibasic acids $C_{2n}H_{2n-2}O_8$ are also changed into hydrurets, provided that they can be heated to 275° in contact with the hydracid. Thus succinic acid yields hydruret of butylene, C_8H_{10}. As Simpson has shown that succinic acid may be prepared from ethylene through cyanid of glycol, we have a process for the methodical transformation of the carburets $C_{2n}H_{2n}$ and $C_{2n}H_{2n+2}$ into $C_{2n-4}H_{2n+6}$.

VI. Alkalies.

Methylamine and ethylamine are converted respectively into marsh gas and hydruret of ethylene:

$$C_2H_2(NH_3)+H_2=C_2H_2(H_2).$$

$$C_4H_4(NH_3)+H_2=C_4H_4(H_2)+NH_3.$$

This gives a new method of resolving the organic alkalies into ammonia and the generating hydrocarbons, and Berthelot believes that it will prove of extensive application in the study of the natural alkaloids.

Part 2d. Aromatic series.

I. Benzol.

The action of the hydracid in this case depends upon its density and the quantity employed. Benzol heated to 280° for 24 hours with 80 times its weight of a solution of iodhydric acid saturated in the cold is almost completely converted into hydrid of hexylene:

$$C_{12}H_6+4H_2=C_{12}H_{14} \text{ or } C_{12}H_6+8HI=C_{12}H_{14}+4I_2.$$

A trace of hydrid of propylene is formed at the same time:

$$C_{12}H_6+5H_2=2C_6H_8.$$

When one part of benzol is heated to 280° C. with 15–20 parts of the hydracid, this reaction is different and is represented by the equation:

$$C_{12}H_6+3HI=C_6H_8+3C_2H+I_3.$$

The chlorinated derivatives of benzol yield with the hydracid benzol, which in its turn with a large excess of the acid gives hydrid of hexylene.

Phenol in like manner reproduces benzol with a small quantity of the hydracid, and hydrid of propylene with a larger quantity.

II. Toluol series.

Toluol when heated with an excess of the hydracid is completely converted into hydrid of heptylene:

$$C_{14}H_8+4H_2=C_{14}H_{16}.$$

But the total reaction is expressed by the equation

$$C_{14}H_8+12HI=C_{14}H_{16}+2H_2+6I_2.$$

With a smaller quantity of the hydracid toluol yields carbon and hydrid of propylene;

$$C_{14}H_8+2HI=C_6H_8+4C_2+H_2+I_2.$$

Benzoic acid heated with 80 parts of a saturated solution of iodhydric acid yields a mixture of the hydrids of hexylene and heptylene. The latter is the normal product, the first being formed from the benzol produced by the spontaneous decomposition of the benzoic acid. The formation of hydrid of heptylene is represented by the equation:

$$C_{14}H_6O_4+7H_2=C_{14}H_{16}+2H_2O_2.$$

With a less quantity of hydracid benzoic acid yields benzol and toluol, of which the latter only represents the normal reaction.

Oil of bitter almonds with iodhydric acid yields toluol and water with small quantities of benzol and xylol, the formation of which the author leaves unexplained.

3. Cumol, xylol and cymol.

Cumol (from coal tar) heated with the hydracid yields hydrid of nonylene:

$$C_{18}H_{12}+4H_2=C_{18}H_{20}.$$

With a less quantity of the hydracid hydrid of propylene is formed:

$$C_{18}H_{12}+6HI=2C_6H_8+3C_2+H_2+3I_2.$$

Xylol in like manner appears to yield hydrid of acetylene $C_{16}H_{18}$.

Cymol (from coal tar) yields hydrid of decylene:

$$C_{20}H_{14}+4H_2=C_{20}H_{22}.$$

III. Azotized bodies.

1. Alkaloids.

Methylamine, &c. Treated with iodhydric acid methylamine is resolved into marsh gas and ammonia:

$$C_2H_5N+H_2=C_2H_4+NH_3.$$

Ethylamine under the same circumstances yields hydrid of ethylene and ammonia:

$$C_4H_7N+H_2=C_4H_6+NH_3.$$

Aniline yields benzol and ammonia; toluidine with an excess of hydracid yields hydrid of heptylene:

$$C_{14}H_9N+12HI=C_{14}H_{16}+NH_3+H_2+6I_2.$$

Methyl-aniline yields ammonia, formene and hydrid of hexylene:

$$C_2H_2(C_{12}H_7N)+6H_2=C_2H_4+C_{12}H_{14}+NH_3.$$

Ethyl-aniline yields ammonia, hydrid of ethylene and hydrid of hexylene. Amyl-aniline yields the hydrids of amylene and hexylene and ammonia.

2. Amids.

Acetamid heated with iodhydric acid yields hydrid of ethylene, ammonia and water:

$$C_4H_5NO_2+3H_2=C_4H_6+NH_3+H_2O_2.$$

Propionitril, C_6H_5N, yields hydrid of propylene and ammonia:

$$C_6H_5N+3H_2=C_6H_8+NH_3.$$

3. Cyanogen compounds.

Cyanogen heated with iodhydric acid yields hydrid of ethylene and ammonia:

$$C_4N_2+6H_2=C_4H_6+2NH_3.$$

This reaction enables us to effect the complete synthesis of hydrid of ethylene from the elements. When cyanid of potassium and iodhydric acid are heated together, a gas is obtained consisting of equal volumes of carbonic oxyd and hydrogen. In this case there are two reactions, represented by the equations:

$$C_2HN+2H_2O_2=C_2H_2O_4=NH_3;$$

$$C_2H_2O_4=C_2O_2+H_2O_2.$$

When gaseous iodhydric acid is heated with cyanid of mercury, the reaction is expressed by the equation:

$$C_2HN+3H_2=C_2H_4+NH_3.$$

This reaction gives a new synthesis of marsh gas.

IV. Complex nitrogenized bodies.

To show the extent of the applications of the new method, Berthelot has studied the action of iodhydric acid upon indigotine and albumine. Crystallized indigotine yields ammonia and a mixture of the hydrids of octylene and heptylene. The author regards the former as normal, the reaction being:

$$C_{16}H_5NO_2+9H_2=C_{16}H_{18}+NH_3+H_2O_2.$$

The hydrid of heptylene results from a decomposition expressed by the equation:

$$C_{16}H_5NO_2+10H_2=C_{14}H_{16}+C_2H_4+NH_3+H_2O_2.$$

Albumine heated with 80 times its weight of the hydracid solution yields ammonia, liquid hydrocarbons of the formene series, a little sulphydric acid and a very large volume of hydrogen. The hydrocarbons begin to boil at 70° C. and rise to a very high temperature. The author has not examined them in detail.—*Bull. mensuel de la Société Chimique de Paris, Jan., Fév., Mars,* 1868.

W. G.

5. *Carbonylic Sulphid.*—BERTHELOT has repeated the experiments of Than upon carbonylic sulphid, an abstract of which appeared in the March number of this Journal, and says "je suis heureux d'en confirmer la parfaite exactitude." He gives in addition, some new and important observations of his own. He finds that potassic hydrate in aqueous solution absorbs carbonylic sulphid gas with a rapidity notably greater than it absorbs the vapor of carbonic disulphid, mixed with air or any other inert gas; although in both cases the absorption proceeds quite slowly.

Liquid bromine and concentrated sulphuric acid act similarly upon both sulphids. Potassic hydrate, moistened with alcohol, absorbs both very rapidly. Absolute alcohol and liquid hydrocarbons dissolve them both with facility. Ammonia, however, either gaseous or in solution, is the best reagent for distinguishing these two bodies from each other. While the vapor of carbonic disulphid reacts only very slowly upon liquid ammonia, carbonylic sulphid gas is absorbed immediately. The vapor of $ЄS_2$ mixed with air, may remain many hours in contact with ammonia gas, without any notable action; $ЄΘS$ on the contrary, being mixed with dry ammonia gas, forms immediately a beautiful crystalline compound which is deposited upon the walls of the containing vessel. The formation of this body is gradual and requires some hours for completion. It results from the union of two volumes ammonia gas and one volume of carbonylic sulphid gas, thus:—

$ЄΘS+(NH_3)_2=\left.\begin{matrix}(ЄΘ)H_2N\\(NH_4)\end{matrix}\right\}S$, ammonic sulpho-carbamate. Theory foresees two remarkable reactions for this substance: 1st, one molecule of water being removed, ammonic sulphocyanate is left:—

$$\left.\begin{matrix}(ЄΘ)H_2N\\(NH_4)\end{matrix}\right\}S-H_2Θ=\left.\begin{matrix}(ЄN)\\(NH_4)\end{matrix}\right\}S.$$

2nd, if one molecule of hydric sulphid be removed, ammonic cyanate (or perhaps urea) is produced:—

$$\left.\begin{matrix}(ЄΘ)H_2N\\(NH_4)\end{matrix}\right\}S-H_2S=\left.\begin{matrix}(ЄN)\\(NH_4)\end{matrix}\right\}Θ\ \text{ or }\ \left.\begin{matrix}(ЄΘ)\,H_2N\\H\\H\end{matrix}\right\}N.$$

Berthelot has succeeded in effecting both these reactions. The first is easily accomplished by heating the aqueous solution of ammonic sulpho-carbamate to 100° C., in a sealed tube. The second succeeds when an aqueous solution of the salt is maintained at a

very gentle heat, in contact with plumbic carbonate. Plumbic sulphid is rapidly formed; and the liquid, when deprived of the excess of lead by H_2S and evaporated on the water-bath, leaves a residue which, on being extracted with absolute alcohol, gives on evaporation a substance possessing all the properties of urea.—*Bull. Soc. Ch.*, II, ix, 6, Jan. 1868. G. F. B.

6. *Test for naphthalin.*—VOHL has published the following reaction, which he says furnishes an exceedingly delicate test for the presence of naphthalin. When this substance is treated with concentrated nitric acid, and the mixture diluted with considerable water, a precipitate is produced, which, after being washed, first with water and then with dilute alcohol (1 part 90 pr. ct. alcohol and 3 water), is placed in a watch-glass with a few drops of potassic hydrate and potassic sulphid, and evaporated to dryness on the water-bath. On moistening the residue with alcohol, a magnificent red-violet color is at once developed.—*J. pr. Ch.*, cii, 29, Sept. 1867. G. F. B.

7. *A Manual of Inorganic Chemistry, arranged to facilitate the Experimental Demonstration of the Facts and Principles of the Science;* by CHARLES W. ELIOT, Professor of Analytical Chemistry and Metallurgy, and FRANK H. STORER, Professor of General and Industrial Chemistry in the Massachusetts Institute of Technology. Second revised edition. New York, 1868. Ivison, Phinney, Blakeman & Co.—In a previous number of this Journal we took occasion to notice the 1st edition of this book. The good opinion then formed from an examination of a portion of the advance sheets has been confirmed and heightened by its use in the class room and laboratory during two academic terms of the Sheffield Scientific School. From no inconsiderable experience we draw the conclusion that chemistry, when taught in the recitation or lecture room merely, is very fascinating to a few and exceedingly unprofitable to the many. The reason of this is obvious. Chemistry has been and still is a science in which the disproportion between principles and facts is great beyond any parallel, and what makes the matter worse is, that the facts are for the most part without logical relations and elude the ordinary grasp of mental associations.

When the learner finds before him an enumeration of the properties of oxygen, to the effect that it is a gas, colorless, tasteless, odorless, of such a density, such refractive power, such specific heat, such radiating power, such solubility, magnetic properties, a supporter of combustion, he has a lesson that may be memorized like a list of words which govern the ablative, but he shortly comes to lose his relish for such mental aliment, especially as no academic traditions exalt this branch of drilling beyond all other means of discipline, and no venerable authority pronounces it indispensable to an education, or rewards its acquisition with a certificate of Master of Arts. We sympathize heartily with the student who fails to be satisfied with this mode of learning chemistry, and cannot look complacently on the pedagogue who stuffs his victims like a Strasbourg poulterer until their hypertrophied memories will contain no

more. There is, to be sure, no royal road to learning, but there is a least difficult and tolerable way, and this is well worth looking after, in these days, when there is so much to acquire. We have never known a class of forty young men to be not merely so satisfied with the study of chemistry, but so eager for it, as that which has lately finished a course in Eliot and Storer's Manual, spending each week two hours in the recitation room and an equal time in the laboratory. The book is not a reduction of some standard treatise executed to order for a publisher's "series," but is the work of accomplished chemists and successful teachers. As a judicious selection and presentation of facts, the book is highly satisfactory. Its crowning feature, however, is its instructions for experimenting. There have not been lacking attempts in the same direction, but here for the first time, we have a sufficient series of experiments, (363) all of which can be made by any student with a trifling outlay for apparatus and materials, and a small expenditure of time, and all of which are reasonably certain of turning successfully, and instructively, by reason of the appropriate minuteness with which the conditions of success are detailed.

As regards the development of "modern" theories, not much has been attempted and in our opinion this is well. The "modern" atomic weights are employed. The laws and consequences of combination by volume, molecular formula and quantivalence are clearly discussed from a conservative yet appreciative point of view. The authors rightly hold that a wide basis of facts and established principles must be laid as a foundation before any eminence in either practical or theoretical chemistry can be thought of. We heartily commend this book to all teachers and students of chemistry as the best we are acquainted with for really learning the elements of the science. S. W. J.

8. *On a mode of extracting the metals Molybdenum and Chromium;* by J. ENEU LOUGHLIN, M.D. (Communicated for this Journal.)—Molybdenum was first prepared by Hjelm in the year 1782. His method consisted in heating the trioxyd of molybdenum in a porcelain crucible for 2 or 3 hours. Several other methods have since been used, prominent among them being that of heating the acid molybdate of potassium; also the reduction of molybdate of ammonium by heat, or the reduction of trioxyd of molybdenum by carbonate of soda. Molybdenum is described as a silver-white metal, not altered by contact with air at ordinary temperature. Sp. gr. 8·5; not attacked by chlorohydric acid or dilute sulphuric acid. Strong sulphuric and nitric acid on the contrary act very powerfully upon it with evolution of sulphurous acid and hyponitric acid. Having had occasion during June, 1867, to use some molybdenum, I tried the methods above stated; they were all very satisfactory as regards the yield of pure metal, but the time was rather long. I then had recourse to the reducing action of cyanid of potassium. Molybdic acid was prepared and tested according to Fresenius, page 179 and 180, Qualitative Analysis. The result being satisfactory as regarded the purity of the molyb-

dic acid, 10 grains of molybdic acid thus prepared were mixed with 15 grs. of cyanid of potassium placed in a porcelain crucible, which porcelain crucible with the lid luted was placed in another crucible, then surrounded by powdered animal charcoal and exposed to a white heat for 12 minutes. At that time the crucibles were removed, allowed to cool, and examined; the porcelain crucible was found lined with a brilliant silver-white metal of a sp. gr. 8·56, which was not attacked by chlorhydric acid, but violently attacked by nitric acid with evolution of hyponitric acid fumes; it reduced oxyd of mercury and oxyd of silver when triturated with these substances. An analysis of this showed it to consist of—

Molybdenum,	98·7
Impurities SiO_2, C,	1·3
	100·0

By the same process, using sesquioxyd of chromium in place of molybdic acid, chromium was obtained possessing a sp. gr. 6·2. The best results were procured by using a reducing mixture of cyanid of potassium and animal charcoal.

II. MINERALOGY AND GEOLOGY.

1. *A System of Mineralogy: Descriptive Mineralogy comprising the most recent discoveries;* by JAMES DWIGHT DANA, Silliman Professor of Geology and Mineralogy in Yale College, etc., aided by GEORGE JARVIS BRUSH, Professor of Mineralogy and Metallurgy in the Sheffield Scientific School of Yale College. Fifth edition, rewritten and illustrated with upward of six hundred wood cuts. xlviii and 828 pp., 8vo. New York, 1868. (John Wiley & Son, No. 2, Clinton Place. $10 in cloth.)—Mineralogy is a growing science, as is very evident from the size of the volume before us. Instead of the 530 pages of the last edition of the work, Descriptive Mineralogy here covers 825 pages, exclusive of an introductory chapter of near 40 pages. Moreover the page of the letter press is large enough nearly for a quarto (4½ inches across) exceeding in area by a fifth that of the last edition. As three-fourths of the volume is in small type and the analyses and chemical formulæ and other parts are presented with the greatest possible condensation and brevity, the covers contain material enough to make four or five ordinary octavo volumes.

The author opens his preface with several reasons for the large size of the work, alluding first to the 14 years that have elapsed since the last edition appeared, and the changes and progress of the science in that time; then to the introduction in this edition of some new features—namely, systematic and detailed descriptions of *varieties* of species; a *historical* synonymy in place of a mere list of names; chemical formulas on the *new system* of chemistry in addition to those on the old; fuller as well as completely revised blowpipe characters;—to which might be added 250 more wood cuts.

In the historical synonymy, as the preface states, "the first author

and first place of publication of a species, and of each name it has borne, and of the names of all its varieties, are stated in chronological order, with the dates of all publications cited; and besides, remarks are added in the text when the subject is one of special interest. The author further observes that "the facts and conclusions have been derived in almost all cases from the study of the original works themselves, and the treatise has thereby become to some extent, a work on ancient as well as modern mineralogy." The historical inquiries here alluded to "were prompted by a desire to place the nomenclature of mineralogy on a permanent basis; they were incident to a search after a reason for choosing one name rather than another from among the number that stand as claimants," in order thereby to escape the increasing confusion in names by applying "the methods which has proved so successful in the other natural sciences, viz: the recognition, under proper restrictions, of the law of priority."

The classification of the species is in system similar to that of the preceding edition, the progress of chemistry and the kindred species not only requiring, in the author's view, no modification of its general plan, but instead giving it new support. Yet in the minor details, the changes made are numerous and important.

The Introductory Chapter supplies such tables and simple information on chemistry and crystallography as were needed to make the volume convenient of use, even for those not familiar with these sciences. Tables of atomic weights are here included, both according to the new and old system; and also multiples of the oxygen percentages by the nine digits, to facilitate calculations. The volume is thereby complete in itself independently of the first part, which we understand will be early issued as a separate work. The introduction contains also sections on nomenclature in which the history and true principles of mineralogical nomenclature are discussed at length; a Bibliography giving a long catalogue of books consulted, and showing that the work is an authority on historical questions connected with the science; and an annotated index to the useful metals and metallic ores.

The title page, as above cited, bears the name of Prof. Brush, and the author acknowledges his important aid as follows in the preface. "In the preparation of this volume, the author owes much to the coöperation of his friend, Prof. George J. Brush. Prof. Brush has had sole charge of the blowpipe department. The pyrognostic characters have been entirely rewritten by him; and while he has had the works of Plattner and von Kobell always at hand, he has, for much the larger part of the species, made personal trials of the reactions before writing them out; so that, although the facts stated are not generally new, they still are mostly from his own observations. His skill also in analytical chemistry, and his thorough knowledge of minerals, have enabled him to remove doubts, afford aid and advice, and furnish new facts on various points throughout the progress of the work. Prof. Brush has also given the proofs, while the work was in the press, the benefit of his revision."

Speaking of the enlargement of the work and the changes it had undergone, Prof. Dana observes that "not a page and scarcely a paragraph of the preceding edition remains unaltered, and full five-sixths of the volume have been printed from *manuscript* copy;" and he adds that, "notwithstanding the impaired state of his health, this manuscript—the paragraphs on the pyrognostic characters excepted—was almost solely in his own hand writing, or in that of a copyist from it, " neither the consultation of original authorities, the drawing of conclusions, nor the putting of the results on paper, having been delegated to another."

The preface closes with the announcement (as a sequel to the statement that the work had been posted up, as far as was possible, to the date of publication) that "a series of Supplements may be looked for, from time to time, in the American Journal of Science." It is to be hoped that the publishers will have an edition of these Supplements as they are issued struck off, that they may be accessible to all who have copies of the mineralogy.

2. *The Mining and Metallurgy of Gold and Silver*; by J. Arthur Phillips, Mining Engineer. London, E. & J. N. Spon, 48 Charing Cross, 1867. 8vo, pp. 532.—This treatise of Mr. Phillips supplies a want long felt in the literature of the mining and metallurgy of the precious metals. The author has enjoyed unusual advantages for the task he has taken in hand, and has produced a volume which contains a great amount of valuable information, and exhibits fairly the present state of knowledge, both theoretical and practical, relating to the subjects discussed. He first describes the principal gold and silver-producing districts, accompanied by such statistical information as could be obtained respecting their yield and importance; and follows this with an account of the methods employed for extracting ores, and lastly with a description of the apparatus and methods made use of for their mechanical and metallurgical treatment.

The author states "that recent observations and experience appear to lead to three important conclusions. First, that the most productive gold bearing rocks are by no means exclusively confined to the Silurian period; secondly, that aqueous agencies have been, and still are, actively at work in the formation of mineral deposits; and, thirdly, that gold ledges are not more liable than ordinary metalliferous veins to become impoverished in depth."

The account of gold mining embraces several important topics not before discussed in a competent manner in any systematic work, accessible to English readers. This is especially true of the peculiarly American system of hydraulic washing, now of cardinal importance in California and Australia, and likely to remain for generations a steady industry, proportioned in importance to the great extent of the deep-lying placers. The account of vein mining is accompanied by a full description of the best approved methods adopted in the most prosperous mines both in California, Brazil and Australia, accompanied with detailed drawings and plates. A particularly interesting account of the Morro Velho mines (commonly called San Juan d'El Rey) and of the methods of

treatment of its ores is given in the chapters on South America (Vth and Xth chapters). These mines are of special metallurgical interest as well from the peculiar character and great amount of the ore operated on, as for the methods of extracting the gold adopted, the carefulness of the management being such that the whole data needed to present a complete exhibit of all the operations of mining and metallurgy are collated and exhibited in the most precise form.*

The ore treated at *Morro Velho* mines is chiefly a mixture of magnetic, arsenical and common iron pyrites, freely disseminated in a quartzose gangue. Calcite, dolomite, brown spar, and, very rarely, copper pyrites are present in the vein. The composition of what is called "pure ore" may be taken at about 43 per cent silica, and 57 per cent pyritous matter. Of these minerals, arsenical pyrites is usually the most auriferous, although it does not occur in large quantities. Pure specimens of this substance afford, by assay, from four to six ounces of gold per ton, and wherever crystals of this mineral make their appearance the yield of the precious metal is large. Cubical pyrites is of more frequent occurrence, but is far less rich in gold, yielding at best but about an ounce and a half of gold per ton by assay. Magnetic pyrites, the most abundant sulphid, is very slightly auriferous, pure specimens yielding but about four dwt. per ton. Branches of clay slate are often found in the principal veins, and this rock, under such circumstances, commonly affords, by assay, from five to seven and a half dwt. of gold per ton. Quartz without any admixture of sulphids has never been found to be auriferous in these veins, and it is a remarkable fact, stated on the authority of Mr. Hockin, managing director of the Company, that the smallest speck of gold is rarely seen, previous to concentration, in any of the ores from this mine. In some parts the vein is cavernous, and less close in its texture than in others; but where drusy cavities are frequent, the yield of gold diminishes; the most productive matrix for gold is a compact mixture of quartz and pyrites with varying quantities of slate. The great metalliferous deposit called the Cachoeira, Bahu, and Quebra Panella, is one continuous, very irregular vein, varying in width from seven to seventy feet, and at one point reaching one hundred feet. The average thickness at the present depth 176 fathoms (1056 feet) perpendicular on the Cachoeira and 165 fathoms on the Bahu, is 19 feet; the sloping space extends over 807 square fathoms. The enclosing rock is a clay slate of tolerably uniform texture. The mineral is brought to the surface on tram ways placed on the slope of the vein, about 45°, in tubs ("kibbles") containing about a ton each. The ore is freed by hand from unproductive slate and is then reduced to fine powder by wet stamping. All the machinery is moved by water power. The pulverised ore, issuing from the stamp coffers through finely perforated copper grates, passes over bullock skins and then lower down over woolen cloths (blan-

* The last accounts which have reached us are information of the almost total destruction of these remarkable mines by a disastrous fire, rendering active operations impossible for a long time to come.

kets) placed on inclined tables. The skins are washed out in vats every hour and the blankets at longer intervals. No mercury is used in the batteries under the stamps, amalgamation being restricted to the concentrated sands from the skins, &c., which are subsequently treated in barrels.

The following table shows the quantity of rock raised and stamped, the amounts of gold produced, and annual net profits made since 1848. This company employs upward of 2,400 hands, from 120 to 130 of whom are Europeans. The number of stampheads at work, is 135 for reducing the ore in the first instance and 56 for restamping residual sand with quartz and slate; arrastres are also used for repulverising the residual sand with good effect.

YEARS.	1849.	1850.	1851.	1852.	1853.	1854.	1855.	1856.	1857.
Stone raised, tons,	67,336	67,106	79,810	82,642	85,698	86,048	87,297	89,877	86,407
Stone & ore stamped, tons,	69,004	64,313	81,629	81,236	86,866	86,433	86,848	87,424	84,325
Gold produced, lbs. Troy,	2,583	2,517	3,057	3,826	3,623	3,464	3,325	2,992	2,539
Net profits, £ stg.	38,136	35,880	51,586	55,391	49,273	44,740	34,466	23,233	787

YEARS.	1858.	1859.	1860.	1861.	1862.	1863.	1864.	1865.
Stone raised, tons,	88,901	88,968	91,361	96,612	90,896	84,758	65,435	76,888
Stone & ore stamp'd, tons,	87,270	82,880	74,528	71,902	67,508	65,697	62,147	59,607
Gold produced, lbs. Troy,	2,733	3,294	3,974	5,051	5,182	4,713	2,852	4,153
Net profit, £ stg.	8,545	38,058	60,460	96,769	87,531	63,285	*	80,438

* Loss, £14,629.

This amount of work was done by 135 stamps striking an average of 55·84 blows per minute (the stamp when new weighing with the lifters, &c., 640 lbs.), for 356·27 days per year, crushing 167·65 tons of stuff per day, or 2,683 lbs. per day per head.

The total value of the precious metals extracted to close of 1865 has been £2,902,480

The total quantity of mineral raised .... 1,769,050 tons.

The average yield of ore .... 4·333 oitavas per ton, or as nearly as possible, half an ounce Troy, value about 32*s.* 6*d.** The ores have on the whole been poor, but the yield tolerably uniform. If the labor had been paid for at California rates of wages the result would have been disastrous. The use of water power and cheap native labor have made the adventure extremely satisfactory. The yield for 1866 exceeded £100,000 net profit.

The details of the processes developed at these extensive mines and mills are of the greatest practical value to all persons engaged in like operations, but are out of place here. A few general results alone must suffice.

Of 1000 parts of ore stamped at Morro Velho, the relative proportions of slimes and fine sand are found to be thus:

Passing a sieve of 10,000 holes per square inch, 88 to 95 per ct.
Not passing a sieve of 2,500 holes, " " 0·28 " 0·50 per ct.

The stamping is thus seen to be extremely fine.

The auriferous material issuing from the stamping mills is associated with gold in three different states, viz:

* An oitava is 2 dwt. 7·348 grains Troy, or 8·67425 oitavas = 1 oz. Troy.

1st. Free gold capable of concentration by washing.

2nd. Free gold in a lamellar form liable to be carried off by suspension in water.

3d. Mechanically combined, gold enclosed in particles of pyrites, but capable of being liberated by further grinding.

The system of concentration adopted at Morro Velho mines, is extremely simple, cheap and economical, consisting of a series of inclined tables or boards 18 inches in width, falling about one inch in a foot, and covered with bullock skins tanned with the hair on, for about 30 feet and followed by other inclines covered by baize or blankets. These inclined sluices are called *strakes*. The following table gives many important details relating to this system as employed at Morro Velho mines:—

Name of Stamps.	No. of Heads.	No. of strakes	Length of strakes		Width of strakes.		Area of strakes, squ're ft.	Squ're ft per ton of ore stamp'd.	Tons ore passing in 24 hours.	No. of skins on strakes	No. of baizes on strakes	Quant'y of head sands pr day.
			ft.	in.	ft.	in.						cub. ft.
Lyon, ...	30	36	31	10	1	6	1,719	46·83	36·73	288	210	20·00
Cotesw'rth	12	13	30	6	1	4½	545	32·21	16·92	104	65	5·50
Susannah,	9	8	27	0	1	6	324	29·61	10·94	48	48	2·75
Herring,..	24	29	35	0	1	6	1,232	35·64	34·56	228	174	18·00
Powles, ..	36	42	33	7	1	3½	1,821	27·55	66·10	336	252	28·47
Addison, .	24	30	31	10	1	5	1,352	38·04	35·54	240	170	17·00
	135	158					6,993		200·79	1244	910	91·72

By this system, about 67 per cent of the gold originally present in the ore is obtained in a highly concentrated state, whilst 33 per cent which escapes is in two distinct forms: 1st, light free gold; 2d, gold enclosed in coarser particles of pyrites.

The first which has been laminated to a great extent by the action of the stamps, exposes too great an amount of surface in proportion to its weight to be saved by any known method and floats off with the slimes. Its quantity is believed to be about 10 per cent of the original amount of gold in the ore. The second is fine gold entangled in pyrites the larger part of which is saved by regrinding and by a subsequent system of *strakes*.

The system of amalgamation of the concentrated ores at Morro Velho is described in detail with careful drawings. It depends essentially on the use of revolving barrels containing 16 cubic feet, or one and a half tons of wet sand, with 60 lbs. mercury and a sufficient quantity of clean water to give the necessary degree of fluidity to enable the globlules of quicksilver formed to become properly incorporated without causing them to become sufficiently mobile to admit of the settling of the mercury and amalgam at the bottom. The barrels are then caused to revolve from 30 to 36 hours, when their contents are discharged into a washing apparatus to separate the slimes from the quicksilver and amalgam, a process which cannot be well explained without a diagram. The loss of mercury in this process on an average of the last three years has been 2·923 oz. per ton of ore stamped. The average cost of extracting the mineral from the mine and its reduction, including every expense of general management, etc., for the last ten years has been 25*s.* per

ton; out of which the cost of stamping alone has been 2*s.* 10*d.* per ton. The average value of the ores stamped is determined by assays of samples of pulp from the batteries; this last is on the authority of Mr. Attwood to the writer, and is not quoted from our author.

The account of the Australian gold mines contains much valuable information not elsewhere accessible in a compact or connected form, but our space limits our extracts to the following interesting tabular exhibit of the approximate yield, and in lbs. Troy of the principal gold-producing countries at the commencement of the present century, and for the years 1850, 1860 and 1865. The return for California and the neighboring States and territories for 1865, is probably somewhat under the truth, since it is exceedingly difficult to obtain the precise yields of Idaho, Montana, Colorado and some other outlying districts. After each absolute sum is given, its relative weight, in comparison with the grand total, produced throughout the world:—

Table showing approximate production of the principal gold fields of the world.

	1800.		1850.		1860.		1865.	
	lbs. troy.	Ratio per cent.	lbs. troy	Ratio per ct.	lbs. troy.	Ratio per ct.	lbs. troy.	Ratio per ct.
Russian Empire,	1,440	2·7	65.600	19·0	66,000	11.3	69,500	12·4
Austrian Empire,			5,600	1·6	5,500	1·0	5,500	1·0
and rest of Europe,	3,500	6·5						
			100	----	350	----	375	----
Southern Asia,	10,000	18·5	25,000	7·3	25,300	4·3	25,000	4 5
Africa,	600	1·2	4,000	1·1	4,000	0·7	4,000	0·7
Chilé,	7,500	13·8						
Bolivia,	1,600	3·0						
Peru,	2,400	4 4						
New Granada,	12,600	23·4	* 34,000	9·9	34,000	6·9	34,000	6·1
Brazil,	10,000	18·5						
Mexico,	4,300	8·0						
California & neighboring States and Territories,	------	-----	208,000	60·2	187,000	31·9	210,000	37·5
Rest of United St'tes,	------	-----	2,950	0·9	1,020	0·2	140	
Nova Scotia,	------	-----	------	----	------	----	2,072	0·4
British Columbia,	------	-----	------	----	20,000	3·4	11,600	2·1
Australia,	------	-----	------	----	217,500	37 0	156,000	27·9
New Zealand,	------	-----	------	----	25,000	4·3	41,400	7·4
	53,940	100	345,250	100	585,000	100	559,587	100

The portion of Mr. Phillips's book devoted to silver offers probably more matter which is new to English readers than the part we have already considered, although as regards the methods of metallurgical treatment its aim is simply to present a fair exhibition of the existing state of the arts therewith connected. The chapters devoted to the silver regions of Nevada and the new mechanical methods of treatment of ores which the experiences of the mines on the Comstock lode have evoked, will be read by all interested in such subjects, with satisfaction and instruction. We must

* The yields of these several States varies considerably from year to year, but the aggregate produce is believed to remain tolerably constant.

reserve for another notice what we have to say on this branch of the subject.

The following table offers an interesting comparison with that already quoted respecting the product of gold. It gives the approximate yield in pounds troy, of the principal silver-producing countries of the world, at the commencement of the present century, and for the years 1850 and 1865. In cases where the returns for the year indicated could not be obtained, the produce for the nearest year for which they could be obtained has been substituted. The quoted produce of the various European countries and of the United States of America may be taken in each instance as a sufficient approximation, but the figures relating to Mexico, Central America and South America, must be regarded as estimates only. A large proportion of the precious metals produced in these countries is annually exported without passing through the hands of government officers, and consequently the most reliable information that can be procured is but little to be depended on. No systematic investigations have been made in Mexico by competent persons, since the date of the writings of Duport and Chevalier.

Table showing the approximate yield of the principal silver-producing countries.

	1800.		1850.		1865.	
	lbs. troy.	Ratio per ct.	lbs. troy.	Ratio per ct.	lbs. troy.	Ratio per ct.
Russian Empire,	58,150	2·5	60,000	2·1	58,000	1·5
Scandinavia,			20,400	0·7	15,000	0·4
Great Britain,			48,500	1·7	60,500	1·5
Hartz,			31,500	1·1	28,000	0·6
Prussia,			21,200	0·7	68,000	1·7
Saxony,			63,600	2·2	80,000	2·0
Other German States,	141,000	6·0	2,500	0·1	2,500	
Austria,			87,000	3·1	92,000	2·2
France,			5,000	0·2	18,000	0·4
Italy,			------	---	* 25,000	0·6
Spain,			125,000	4·4	110,000	2·8
Australia, New Zealand, British Columbia, Nova Scotia,	--------	----	10,000	0·4	9,500	0·2
Chili,	18,300	0·8	238,000	8·4	299,000	7·3
Bolivia,	271,300	11·6	130,000	4·6	136,000	3·3
Peru,	401,850	17·2	303,150	10·7	299,000	7·4
New Granada,	5,000	0·2	18,000	0·5	15,000	0·4
Brazil,	1,200	---	·675	---	1,500	0·4
Mexico,	1,440,500	61·7	1,650,000	58·4	1,700,000	42·3
United States,	--------	----	17,400	0·7	1,000,000	25·0
Total,	2,337,300	100	2,827,425	100	4,017,000	100

We cannot close this notice without thanking Mr. Phillips for the very able manner in which he has performed the task he has undertaken, and for bringing together in so readable and well designed a form, so much information of permanent value upon the subjects of which he treats. The book is beautifully printed and illustrated, the only regret being that these very excellencies should place it beyond the reach of a large class of readers. B. S.

* Obtained from the island of Sardinia, where it is found associated with galena.

3. *New Geological Maps and Chart.*—Prof. JAMES HALL is engaged upon a geological Map of the United States, which he informs us, will be essentially a continuation of the large map of the Canadian Survey, soon to be published. The Northern States will in fact be printed from the same plates, and the portions south and west will be continued by Prof. Hall on the same scale.

We understand also that Prof. O. C. Marsh, of Yale College, has in preparation a pocket geological map of the United States, designed for general use among students of geology. This map is intended to be a companion to Dana's Manual of Geology, and it will be issued by the same publishers.

A Geological Atlas of North America is also in course of preparation under the direction of Prof. C. H. Hitchcock, of New York. It will contain a series of maps of the different States and Territories, as well as of the British Provinces, and will soon be published in New York.

Prof. A. Winchell, of Ann Arbor, has, moreover, in progress a Geological Chart, designed to illustrate the more prominent facts of geology, which will doubtless prove of great value to teachers and students of the science.

4. *New borate from Mine Hill, Franklin, Sussex Co., New Jersey—Sussexite;* by G. J. BRUSH.—A new asbestiform borate has been found at Franklin, which I propose to describe in the next number of this Journal. The specimens were obtained at the locality by Mr. Wm. G. Mixter, of the Sheffield Laboratory.

III. BOTANY AND ZOOLOGY.

1. *The Variation of Animals and Plants under Domestication;* by CHARLES DARWIN, M.A., F.R.S., &c. American edition. 2 vols. 1868. (Orange Judd & Co., New York).—In his "Origin of Species" the author promised to give us some of the details of the changes produced in animals and plants by Man's selection, and this promise is redeemed in the work before us. The first volume is devoted to a consideration of breeds and varieties of various domestic animals and plants; the second relates to the variability of species in nature, the inheritance, crossing, hybridism, methodical selection, &c., the whole concluding with certain hypotheses and speculations.

The first volume will perhaps be most prized by naturalists, as it treats mostly of unquestioned facts concerning varieties, their characteristics, history, &c., but so arranged that they continually suggest and support the author's theory.

The second volume, it seems to us, will be most valued by practical breeders of animals and cultivators of plants, men whose professions have made them familiar with the class of facts recorded in the first. We will not at this time consider the author's well known views respecting species, and their origin, as the hypotheses and speculations referred to can hardly be discussed, or even explained, within the narrow limits of the present notice. At

present we restrict our attention to some observations recorded in the first volume.

Naturalists have generally assumed that the different breeds of domestic cattle constitute but one species; and the same of dogs, horses, sheep, &c., while practical breeders of each have as generally assumed the contrary. The author after a careful consideration of the facts, concludes that cattle, dogs and hogs are each derived from more than one wild species; but that horses, rabbits, pigeons, fowls, &c., have each descended from but one parent species. The evidences given in favor of these views are rather historical than in anything indicated by the characteristics of the breeds themselves; for there is a greater difference between extreme breeds of pigeons than those of cattle, and the difference between the different breeds of horses, is greater "than between the six or seven other living species of the genus *Equus*."

We are led to infer that he believes that sheep also have not descended from a single species. It is certain that by far the majority of successful sheep-breeders are of this opinion; and after stating the views of various authors upon this subject, some of whom believe that all the breeds are derived from a few original species, and others from many, he adds, "under such a hopeless state of doubt it would be useless for my purpose to give a detailed account of the several breeds." We regret that he did not consider the variation of some breeds within known periods. The histories of English breeds furnish ample materials. The honest and careful observer, who will record the experience of American sheep breeders within the last eighty years, will do a great service in our Natural History. The changes that have been effected step by step, in the improvement of the "Infantado" merinos in late years, the changes wrought in the Saxon and other breeds a few years ago, the effects of transplanting well marked breeds from the hills of the extreme Northern States to the plains of Texas and California, are not only subjects of immense practical importance to our industry, but also embrace a class of facts which ought not to be lost to science. Mr. Darwin treats at some length of domestic rabbits, agreeing with other naturalists, that they are all descended from the wild rabbit of Europe. We are all familiar with the remarkable difference between the breeds, some being four or five times as large as others; in color, habit and disposition, they are not less diverse. Mr. Darwin finds corresponding changes in the bony and nervous system. Paleontologists would consider the skulls of the different breeds to indicate well marked species, if they were found fossil. In the half lop-eared breeds the skull has a lateral curvature; in most breeds the capacity of the brain cavity has decreased relatively to the size of the animal or to the length of the skull. He gives a curious table of measurements illustrating this. In one breed this decrease is 23 per cent, considered relatively to the weight of the animal, and 54 per cent relatively to the length of the skull. He says, "I found on comparing the skulls of ten species of hares in the British Museum, that they differed from each other chiefly in the very same points in which domestic rabbits

vary,—namely, in general proportions, in the form and size of the supraorbital plates, in the form of the free end of the malar bone, and in the line of suture separating the occipital and frontal bones." Other parts of the skeleton show equal variation.

The varieties of domestic pigeons are described in detail; but any abstract of these chapters would be unsatisfactory. They constitute an important part of the volume, and must be read entire.

Besides mammals and birds, a few fishes and insects are considered. If the statements of bee-keepers in this country can be credited, they have attained a much greater success in breeding to points, than is here claimed for European experts.

Mr. Darwin's theories have perhaps been more kindly received by botanists than zoologists, and yet we find in the work before us, (as in his "Origin of Species.") much more space devoted to the variations of animals than of plants, in fact, but a fourth of this volume is devoted to the latter. Perhaps, like the shrewd lawyer, he elaborates his argument most where it is most wanted. Doubtless the facts relating to the geographical distribution of plants have had great weight with botanists; but it is equally true that plants vary much more than animals; whether these variations are more remarkable or not, they are certainly no less striking.

We are reasonably certain what wild species were the parents of most of our domestic animals, and also of birds. "There is in fact, only one kind of domesticated bird, namely, the Chinese goose or *Anser cygnoides*, of which the parent form is said to be still unknown or extinct." Far different is it with plants. The original wild forms of many have either become extinct, or their cultivated progeny have varied so widely that it is no longer possible to recognize their parentage. In 1855, DeCandolle gave a list of 157 of the most useful cultivated plants. Of these he believes that 85 are almost certainly known in their wild state, but on this head other competent judges entertain great doubts. Of 40 of them, he admits that the origin is doubtful, and 32 are ranked as quite unknown in their aboriginal condition. This is a large proportion. One fifth are admitted to be of unknown origin, and nearly one half doubtful. This list does not include ornamental plants, nor many useful varieties which present ill defined characters.

We cannot notice variations of plants further than of certain cucurbitaceous species, which have been so extensively investigated by Mr. Naudin, who considers *Cucurbita pepo* to be the most variable plant in the world. "The fruit of one variety exceeds in volume that of another by more than two thousand fold," and the other variations are as remarkable. Some varieties are slender vines, others erect, some with tendrils and others without. There is not an important organ that has not undergone great variations in some one or another of the varieties, and to describe all these variations would require a large volume, and yet, each of these modified forms is reasonably permanent in cultivation. Melons and cucumbers vary nearly as much. Concerning these variations, Mr. Darwin pertinently quotes Mr. Naudin's remark, that "this extraordinary production of races and varieties by a single species,

and their permanence when not interfered with by crossing, are phenomena well calculated to cause reflection."

Apropos to this family we notice an unaccountable mistake on p. 430, where, as on the corresponding page in the English edition, the water-melon is said to be *Cucurbita moschata!* This entails a misunderstanding on p. 432, where the *C. moschata* spoken of is the squash to which that name belongs, and perhaps also on the following page, where the "endless varieties of the melon" (meaning of course musk-melon, *Cucumis Melo*) are referred to. If we rightly remember, an erratum in the first English edition corrected this wrong name of *Cucurbita moschata* into *Citrullus vulgaris*, the proper name of the water-melon; but it seems to have been overlooked in the reprinting on both sides of the water.

This "authorized" American edition is from the second English edition, containing the corrections and additions in that, together with some new corrections by the author. It has a preface by the author, and an introductory note by Prof. A. Gray. It is reasonably well printed, and furnished at less than half the cost of the English edition. W. H. B.

2. *A second critical notice of Alcyonaria in the Museum of Yale College*, etc.; by A. E. VERRILL.—Since the publication of the "critical remarks" in the last number, the writer has had opportunities for the reëxamination of several additional species, including some in the Collection of the Essex Institute, Salem, Mass., the Boston Society of Natural History, and the Museum of Comparative Zoology. Some of the more important points noted are here presented. *Echinogorgia* sp.; = *Antipathes flabellum* Esper (non Pallas). A species belonging to this genus, obtained by Capt. W. H. A. Putnam at Singapore, and received from the Essex Institute, is reticulated in a manner precisely similar to *Paramuricea cancellata* V., from which it can scarcely be distinguished by the axis alone, unless by its somewhat darker color. It agrees exactly with Esper's *Antipathes flabellum* (Tab. I), which is more likely to be this species than the former. Specimens of an *Echinogorgia* from Ceylon (?) referred to *E. furfuracea* (Esper sp.) approach these specimens and may be the same species. The supposed Ceylon specimens are accompanied by two other species of *Echinogorgia*. One is *E. sasappo* Köll., the other is an apparently undescribed species, with rather thick dichotomous branches and large, slightly raised verrucæ, and having large and very rough spicula.

Plexaura friabilis (Lamx.?) Verrill, Proc. Essex Inst., iv, p. 186. This species has very peculiar spicula, quite different from those of any West Indian species. They resemble most the spicula of some species of *Plexaurella*, and this species will have to be referred either to that or a new genus. Whether it be really the species described by Lamouroux is still somewhat uncertain, but to me this, rather than any West Indian form, seems most likely to be his species.

Plexaura turgida Verrill, Bulletin M. C. Z. (? Ehrenberg sp.) This proves, on microscopic examination of the spicula, to be a true *Plexaura*, and not, as previously supposed, identical with *Plexaurella crassa* Köll.

Litigorgia stenobrachis V. (Val. sp.). By some unaccountable mistake in the examination of the spicula, this species, together with *L. flexilis*, *L. rigida* V., and *L. cuspidata* V., were erroneously referred to *Eugorgia*, in the last number of this Journal.

"*Gorgonia humilis*" Verrill, Memoirs Boston Soc. Natural History, vol. i, p. 6, 1864 (non Dana). A re-examination of this species has convinced me that it is identical with *Litigorgia ramulus* from Panama. Its supposed locality (Charleston, S. C.) is probably erroneous, since a Panama species of *Astrangia* was found attached to the base of one of the specimens.

Gorgonia albicans Kölliker. The specimen from False Bay Cape of Good Hope, referred to by me in the Proceedings of the Essex Institute, vol. iv, p. 187, 1865, as a white variety of *Lophogorgia palma*, proves to be the species recently named by Dr. Kölliker. It is Esper's *Gorgonia palma, var. alba*, Tab. xl.

Note.—In the last number of this Journal, p. 417, the name of the new genus of Starfishes referred to should read *Amphiaster* instead of *Amblyaster*.

3. *Note on Ethmophyllum and Archeocyathus;* by F. B. Meek. (From a letter to J. D. Dana, dated Springfield, Illinois, June 11, 1868.)—Since preparing my remarks, published in the Journal of Science (Jan. number, p. 62, 1868) on the curious fossil from Nevada, for which I proposed the name *Ethmophyllum*, I have been led, by further comparisons, to think it probably not generically distinct from *Archeocyathus* of Billings.* At any rate, it seems to agree *very* closely in internal structure with his *A. Minganensis* and *A. profundus.* The Nevada species differ so widely in form and general appearance, as scarcely to suggest a comparison with Mr. Billings's species, and besides, I had derived my impressions of his genus entirely from his typical species, *A. Atlanticus*, which also differs so materially in internal structure, that Mr. Billings suspected it might be generically distinct from his *A. Minganensis.* If these types are generically identical, however, I can scarcely entertain a doubt but that the Nevada fossil will fall into Mr. Billings's genus, which has priority of date. In this case, the names of the Nevada species would become *Archeocyathus Whitneyi*, and *A. gracilis.*

Since examining Mr. Billings's figures of the analogous species, *A. Minganensis* and *A. profundus*, I am inclined to agree with him that these fossils belong to the *Protozoa*, and are not true corals. If so, it is possible that what appeared in sections to be a minute vesicular tissue filling the central cavity, and interseptal spaces, in the Nevada species may have been sections of minute spicula. It is also worthy of note (as may be seen by reference to my remarks in the January number of this Journal), that I was strongly impressed with the similarity of some of the internal characters of the Nevada species to some types of *Foraminifera;* while Mr. Billings states† that Prof. Dawson thought his types agreed in microscopic structure with the *Foraminifera.* If they belong to the latter group, however, the apparent spicula can scarcely be such.

* New species of Lower Silurian Fossils, p. 3, 1861.

† Palæozoic fossils, p. 356.

IV. ASTRONOMY.

1. *Theoretical Astronomy relating to the motions of the Heavenly Bodies revolving around the Sun in accordance with the law of Universal Gravitation*, &c.; by JAMES C. WATSON, 8°, pp. 662. 1868. Philad., J. B. Lippincott & Co. London, Trubner & Co.—In the first chapter of this work the author establishes briefly the equations of force and motion, and then, neglecting the perturbing forces, develops from these the various formulæ that refer to the motion of a body in a conic section, first, for its heliocentric and then for its geocentric coördinates. The equations for the corrections for aberration, parallax, precession, and nutation, are also given in full, and the whole is illustrated by a numerical example. Numerical examples also illustrate the formulæ and methods in every chapter of the volume, except the sixth and seventh.

The second chapter consists of an investigation of the differential formulæ which express the relation between the geocentric, or heliocentric places of a heavenly body, and the variation of the elements of its orbit. All the cases that are likely to occur in practice seem to be fully provided for.

The third, fourth, and fifth chapters give the formulæ for the direct computation of orbits. The third chapter is devoted to parabolic orbits, and the methods of computing them from three complete observations. As the six coördinates from three complete observations are more than sufficient to determine the five elements of a parabolic orbit, a method for correcting the elements by the variation of one of the extreme geocentric distances naturally follows in this chapter. The fourth chapter is devoted to orbits whose eccentricities are not known, and the determination of their elements from three complete observations. The fifth chapter considers the same problem where four observations are given, of which the second and third are complete, and the longitudes of the first and fourth are given, a solution required for orbits of small inclination to the ecliptic, and useful in some other cases.

The sixth chapter contains an investigation of various formulæ for the correction of the approximate elements of an orbit. These are the formulæ for computing a circular orbit from two observations, for correcting an approximate orbit by the variation of two geocentric distances, for correcting by the variation of the node and the inclination of the plane of the orbit, for correcting by the variation of one geocentric distance, the formulæ expressing relations between two places in the orbit, and those for correcting the orbit by the variation of the major axis or of two radii vectous.

The method of correcting by the variation of one geocentric distance is peculiar in that it depends upon a comparison between the differential equations of motion in an undisturbed orbit and the direction and curvature of the apparent path of the body in the heavens. It is an ingenious application of the method given by Laplace for the original computation of an orbit.

Among the formulæ for the relations of two places in the orbit those for the chord of an elliptic or hyperpolic arc in terms of the extreme radii vectores, and of the time of describing the sector, are especially worthy of attention. The methods derived therefrom for correcting elliptic orbits of great eccentricity must be useful to the computer.

The seventh chapter contains an elementary presentation of the method of least squares, and a very full explanation of the best methods and corresponding formulæ for applying it in the correction of the elements of orbits, under the various circumstances likely to occur in practice.

The final chapter of 130 pages consists of an investigation of the various formulæ for computing the special perturbations of a heavenly body, and has doubtless required more labor from the author than any other portion of the volume. Four methods are given of computing perturbations according as we consider the elements or the coördinates to vary. Special cases of comets follow, in particular that in which the comet may be regarded as moving about the common center of gravity of the sun and the disturbing planet, and that in which it comes very near to the planet. The effect of a resisting medium in space and the methods of computing it, close the chapter.

In almost all parts of the volume, but especially in the earlier portions, the author is travelling over beaten paths, yet in every chapter there are new formulæ and methods. The preparation of such a work would be impossible by one who has not been long familiar with the computation of orbits. For its original matter as well as for the elegance of its methods and formulæ it cannot fail to be for a long time a standard work in Astronomy. It is a high honor to American Astronomical Science to have produced two such works as those of Chauvenet and Watson. H. A. N.

2. *Astronomical and Meteorological Observations made at the United States Naval Observatory during the year* 1865. By the authority of the Secretary of the Navy, Rear Adm. C. H. Davis, U. S. N., Superintendent. Washington, 1867, 4to, pp. xli, 47, 29 and 550.—Together with the Astronomical observations made during the year by the several instruments, there are given the resulting right ascensions and declinations reduced to 1870, along with a general catalogue of the 2787 stars observed. The meteorological observations were made every three hours, and with them are given the mean monthly and annual results. Two very valuable appendices are included in the volume. The first is a description of the new transit circle and its adjuncts, with the methods for obtaining their errors, and the application of these methods to this instrument. The second is an investigation of the distance of the sun, and of the elements which depend upon it, by Prof. Newcomb. This consists of a discussion of about 300 observations of Mars in 1862 at places in the northern and southern hemispheres. The result is $8''{\cdot}855 \pm {\cdot}020$ for the value of the solar parallax. Combining this result with other accredited determinations of this important constant he obtains $8''{\cdot}848 \pm {\cdot}013$, corresponding to

a mean distance of 92,880,000 statute miles. From this results the value 326800±1360 for the mass of the sun, (taking the mass of the earth as unity) and 1÷(81·44±0·33) for the mass of the moon. The resulting velocity of light would be 185,600 miles per second.

3. *Small Planets.*—A small planet (95) was discovered by Dr. Luther at Bilk on the 23d of November. He has given it the name *Arethusa.* Another planet (96) was discovered by Mr. Coggia at Longchamp-Marseilles, on the 17th of Feb. On the same night Mr. Tempel, at Marseilles, added another to the list, (97), *Clotho.* He requests discoverers of the next two planets to accept for them the names *Lachesis* and *Atropos*, that the three sister Parcæ may together complete the first hundred (?) minor planets.

Upon the announcement in the Academy of Sciences at Paris by Mr. LeVerrier, of the discovery of the planet (96), exception was again taken by Mr. Delaunay to the reprehensible practice of withholding the names of the subordinates in the government observatories by whom discoveries of planets and comets are made. He added that the discoverer of planet (91), Aegina, was Mr. Borelly. The scientific world will sustain Mr. Delaunay, probably without a dissenting voice.

Prof. Watson has given to the two planets discovered by him the names, *Minerva* for (93), and *Aurora* for (94).

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Petroleum Wells in Mexico.* (From a letter addressed to Smithsonian Institute by BENJ. CROWTHER, Esq., and communicated to this Journal by Prof. HENRY.—Proceeding down the river Tuxpan in the direction of its mouth emptying into the Gulf of Mexico, and when within a mile and a half of the same, we entered the mouth of the Lagun Tampamachoco situated on the north side of Tuxpan river. From thence we steered our course generally in a north-northwest direction and landed at a point on the west bank of the Lagun, the whole distance travelled being about 20 miles from the town of Tuxpan. There is a very short water communication between this Laguna and the Rio Tampico upon which stands the city of that name.

A Mexican friend and guide landed at the point just named and proceeded to explore the best route by which to conduct us to the wells. He returned in about an hour, bringing a handfull of thick black tar, of the consistence of putty. He reported the path to be over land everywhere swampy, and advised us not to attempt to proceed, as we would be able to reach the locality only by wading bare-footed through marshes with very slippery bottom and beset with prickly and stubble undergrowth. But concluding to finish the adventure, our boots, shoes, &c., were left in the canoe, and with pants knee high we followed our guide in a northwest direction for nearly two miles. When within three-fourths of a mile of the wells, the surface of the ground was found to be covered with a continuous cracked scab of bitumen, having beneath it an ash colored soapy clay soil, upon which it was difficult to stand or walk.

Arrived at the wells, we found the principal one to cover about fifteen feet in circumference, enclosing a black and brown tarry mass and bubbling up in the center at intervals. A stick thrust into the crust of tar disappeared out of sight. About 150 yards in a northeast direction another well was found very similar in all respects; also another about 200 yards westerly from the first, and finally two others in a northeast direction, all of them bearing a general resemblance, and probably fed by the same subterranean source. Contiguous to the five openings here enumerated, two others were descried at no great distance. We concluded that not far below the surface of the ground and below the bed of the Lagun and extending into the gulf, (here about 5 miles distant) there exists a great petroleum deposit. The substance is called Chapapote by the Mexicans, and is used for paving the bottoms of canals, and painting sea-going vessels. In the Gulf of Tampico the petroleum or coal oil tar boils up in the middle of the Lagun, and large cakes of this substance in its inspissated form are floated ashore near Galveston and the coast of Texas.

2. *American Association for the Advancement of Science.*—The next meeting of the Association will be held at Chicago, during the week commencing on Wednesday, Aug. 5, 1868, at 10 o'clock A. M. A large and interesting meeting is expected. Arrangements have been made with the railroad companies, by which return tickets will be furnished free to those who attend the meeting.

Members, and those who wish to become members, are requested, on their arrival in Chicago, to register their names at the offices of the Local Committee, Parlors Nos. 3 and 4, Sherman House, where they will be furnished with all information which may be desired, in regard to accommodations, etc. J. Young Scammon is Chairman, and William Stimpson, Secretary of the Local Committee.

3. *British Association.*—The British Association holds its next meeting at Norwich, commencing Aug. 19. Joseph Dalton Hooker will preside.

OBITUARY.

Anton Rosing.—ANTON ROSING, of Christiania, was born in 1827. A sketch of his life has just been published in the *Norsk Landmandsbog* for 1868 by P. C. Asbjörnsen, well known in English literature through translations of his Norse Tales, and to American Naturalists through the citations of Mr. Marsh.

Rosing studied in Copenhagen with Oersted, and was afterward for several years chemical manager of the gas works at Christiania, and editor of the Polytechnic Journal of that city. In 1855 he was appointed by the Norwegian Government to a new chair of Agricultural Chemistry, with liberty to travel during several years. He crossed at once to Germany and studied there assiduously as well as in several Parisian laboratories.

As a scientific investigator, Rosing has done good work, and he has also written largely. Those who have known him well, however, will remember him less for these things than for the high aims he cherished, and for his remarkable power of influencing his fellow men. Of Rosing it may be said that, though an earnest student, his great

fund of information and his proficiency in all things pertaining to chemistry seemed to be less intimately connected with laborious study than with his cordial sympathy for the labors of others, his kindliness and liberal views which endeared him to a multitude of chemists. He was a welcome guest in every laboratory, and a leading spirit wherever his lot was cast. As a speaker, he was singularly eloquent, while he showed with many of his countrymen that power over foreign tongues which sets Babel at defiance.

In Paris, a poor foreign student, supported by no patronage either of University, church or science,—his influence was felt to a remarkable degree. One of the founders of the Chemical Society of that city and its first President, to Rosing more than to any other man is doubtless to be attributed the tone and character of the earlier publications of this most distinguished Society. In that distant future when cosmopolitan journalism shall have become an accomplished fact, the *Répertoire de Chimie pure et appliquée* will be remembered as a bold, and, all things considered, a remarkably successful attempt to associate, in a purely scientific enterprise, the men of many lands. Torn by internal dissensions, of the home management in which, let it here be said, the foreign collaborators had happily no part, the *Répertoire* was merged, after five or six years, in the existing "*Bulletin*" of the Society, and the distinctive cosmopolitan character which Rosing had imparted to it was lost. As is well known, the *Bulletin* is still conducted with great ability, and takes rank as the best among chemical journals; but there are chemists not a few, and the *Rédacteur en chef* is of them, who lament the day when its now purely Gallic style and savor was tempered, if not improved, by an infusion of outer barbarism.

From Paris, Rosing passed into England and Scotland, studying chemistry and agriculture, in the interests of his chair, until stricken with palsy in 1859. He died at Christiania on the 29th of March, 1867.

Few chemists,—no matter how highly placed,—have succeeded in exerting a stronger personal influence, or in attaching to themselves so large a circle of intimate and devoted friends. Not only in the Scandinavian capitals, in St. Petersburg, and in many a German town, but in Paris, Edinburgh, Vienna, Parlermo, Milan and in more than one American city, the memory of Rosing will be cherished, and his untimely death deplored. F. S.

Prof. Julius Plucker, the physicist, of Bonn, recently died, aged sixty-seven years.

Dr. Charles Grafton Page, well known to all readers of this Journal as an original investigator in electro-magnetism, died in Washington, D. C., May 5, 1868, aged 56 years. A notice of his life will appear in another number of this Journal.

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *A Treatise on Meteorology; with a collection of Meteorological Tables.* By Elias Loomis. 8vo, 1868. Harper and Bros., New York.—This work is intended to serve both as a text-book for instruction, and at the same time as an exposition of the most

important results of recent researches in Meteorology. The student of this science has hitherto been unable to find in a single volume its principles developed with reasonable minuteness, and at the same time a presentation of the latest investigations. The present volume adequately supplies their want. The selection and arrangement of material is judicious, and the statements are lucid.

The special student would probably prefer a more liberal reference to the names of those to whom we are indebted for recent acquisitions in this department of science. If this be a defect in a volume designed primarily as a class-book, it is in part compensated by a list of works and memoirs on Meteorology, at the end of the volume. The last chapter gives the present state of our knowledge of shooting stars, meteors, and aërolites.

At the end of the volume are thirty six tables of various kinds, useful both to the observer and to the investigator.

2. *The Butterflies of North America*, with colored drawings and descriptions; by Wm. H. Edwards. Part I, 4to, with five plates. Philadelphia: published by the American Entomological Society. April, 1868. Price $2.00 per part.—Hitherto every one who has been at all interested in this attractive group of insects must have felt the great need of reliable works with accurate colored illustrations, especially of the critical and rare species. The two principal illustrated works, hitherto published, are not only very incomplete, but rare and costly, and almost inaccessible in this country, except, perhaps in the principal city libraries.

This new work promises to fill this want in a truly admirable manner and at a price that makes it available for every one. The first number contains descriptions and beautiful illustrations of *Argynnis Diana*, *A. Cybele*, *A. Aphrodite*, *A. Nokomis*, and *A. Atlantis*. Both surfaces are represented, and, in nearly every species, both sexes. The illustrations are life size and drawn with remarkable accuracy and beauty, and the coloring is very perfect and life-like. It is proposed to continue the work by issuing a number every three months, each number containing five plates, and complete in itself. The species selected for illustration will be chiefly those not hitherto figured, or only imperfectly or erroneously represented by previous authors. The letter-press is well printed in large type, on fine paper. The work, when completed, will form an attractive volume.

3. *Observations on Polyzoa, Suborder Phylactolæmata*, with nine plates; by Alpheus Hyatt. From the Proceedings of the Essex Institute, Salem, Mass., 1866 to 1868.—In this country the very interesting group of fresh-water *Polyzoa* has hitherto attracted but little attention, even from professional naturalists. Dr. J. Leidy is the only American author who had previously written upon them. In the present extended memoir Mr. Hyatt has described very carefully and minutely the anatomical structure of *Fredricella*, *Plumatella*, *Pectinatella*, and *Cristatella*, and has brought out with remarkable clearness, the various anatomical details by means of greatly enlarged outline figures. The various species and their varieties are also figured of natural size, showing the modes of

budding and forms of the colonies. The statoblasts or winter-buds of various species are illustrated by wood-cuts in the text, and afford valuable characters for the discrimination of the species. The last part of the work is devoted to descriptions of the genera and species observed by the author. These are three species of *Fredricella*, four of *Plumatella*, one of *Pectinatella*, and one of *Cristatella*. Of these, five are new species.

This monograph really supplies us with a nearly complete manual of all the known North American species. V.

4. *The Workshop*: A Monthly Journal devoted to the progress of the useful arts. Edited by Prof. W. BAUMER, J. SCHNORR and others. 4to, Nos. 1—4. (E. Steiger, 17 North William st., N. Y.)—This beautifully illustrated journal addresses itself especially to those engaged in the decorative arts. It furnishes carefully drawn examples of the best productions in ancient and modern art, accompanied with working plans on a larger scale to guide the artist in reproducing the designs. Original articles on style in connection with industry, and on art instruction, and reports on raw materials and new processes, accompany the descriptions of special designs. This timely journal may be considered an outgrowth of the Paris Exhibition of 1867. It is of peculiar value to the American art worker in whatever department of art industry he may be engaged, and is furnished at the moderate cost of $5.40 per annum.

5. *Transactions of the American Entomological Society, Philadelphia*, vol. i, No. 4.—This number closes the first volume of the Transactions of this Society. The memoirs of the volume are all of high merit, and the illustrations, of which there are many plates besides wood-cuts, excellent. No. 4, finishes the Geotrupes of Boreal America by G. H. Horn; and includes, after this, Descriptions of American Lepidoptera, with two colored plates, by A. R. Grote and C. T. Robinson; Notes on the Apple Bark-Louse (*Lepidosaphes conchiformis*), with a description of a supposed new Acarus, by H. Shimer; Catalogue of a small collection of Hymenoptera made in New Mexico in 1867, by E. T. Cresson; Catalogue of works in the Library of the Society.

Proceedings of the American Association for the Advancement of Science, 15th meeting, held at Buffalo, N. Y.. Aug., 1866. 130 pp. 8vo. Cambridge, 1867.

On the Use of the Barometer on Surveys and Reconnaissances: Part I, Meteorology in its connection with Hypsometry. Part II, Barometric Hypsometry. Submitted to the Chief of Engineers, U. S. A., by R. S. Williamson, Major, Corps of Engineers, etc. Professional Papers of the Corps of Engineers, U. S. Army, No. 15. 248 pp. 4to, with many plates. 1868. (New York, D. Van Nostrand.)—Also, as Appendix to the above, Practical Tables in Meteorology and Hypsometry, by id. 156 pp. 4to.

The American Annual Encyclopedia and Register of important events of the year 1867. Vol. vii. New York. 8vo, pp. 799. (D. Appleton & Co.)

Electro-Physiology and Therapeutics; being a study of the Electrical and other Physical Phenomena of the Muscular Systems during health and disease, including the phenomena of the Electrical Fishes, by (the late) Charles E. Morgan, A.B., M.D. 8vo, pp. 714. New York, 1868. (William Wood & Co., 61 Walker st.)

The Elements of Physiology and Hygiene, a Text Book for Educational Institutions; by Thos. H. Huxley, LL.D., F.R.S., and Wm. Jay Youmans, M.D., with numerous illustrations. New York, 1868. 12mo, pp. 420. (D. Appleton & Co.)

A Guide to the Study of Insects, and a treatise on those injurious and beneficial to crops; for the use of Colleges, Agriculturists, etc.; by A. S. Packard, Jr., Salem. Issued in parts. Part I, 60 pp. 8vo. Promises to be a popular work of great value. Illustrations 500 or more, and excellent. Subscriptions are desired.

Lessons in Elementary Chemistry: Inorganic and Organic, by Henry E. Roscoe, B.A., F.R.S., Prof. of Chemistry in Owens College, Manchester. New York, 1868. (Wm. Woods & Co., 61 Walker st.

The Magnetism of Ships and the Deviations of the Compass. A series of papers from the transactions of Foreign Societies, by Poisson Airy, Archibald Smith, Fredk. John Evans, R.N., W. W. Rundell, &c., reprinted by order of the Secretary of the Navy, under the direction of Commodore Thornton A. Perkins, U. S. N., Chief of the Bureau of Navigation. Edited, and the memoir of Poisson translated by B. Franklin Greene, C. E. 860 pp.

Production of Iron and Steel in its economic and social relations, by Abram S. Hewitt, U. S. Commissioner to the Univ. Expos. at Paris, 1867. 104 pp. 8vo. Washington, 1868.

Notes on the Geology of the Survey for the extension of the Union Pacific Railway, E. D., from the Smoky Hill river, Kansas, to the Rio Grande; by John L. LeConte, M. D. 68 pp. 8vo. Philadelphia, 1868.

Monograph of the Terrestrial Mollusca of the U. States, by Geo. W. Tryon, Jr. Over 200 pp. with 18 lithogr. plates crowded with figures. A few copies on sale by the Conchological section of the Acad. Nat. Sci. Philadelphia; pr. uncolored \$8,50; colored, \$13.50; with duplicate plates, colored and tinted, printed on plate paper, \$20.00. Address E. J. Nolan, M. D., Treas. of Publication Committee.

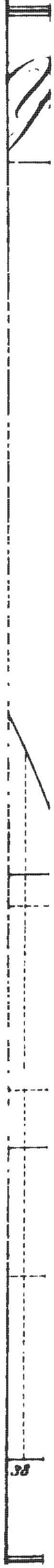
Report on the Bahamas' Hurricane of Oct., 1866, with a description of the city of Nassau; by Gov. Rawson, C. B. Nassau, (E. C. Moseley.)

Die Fossilen Mollusken des Tertiær-Beckens von Wien, von Dr. Moritz Hörnes. Vol. ii., No. 7, 8, Bivalves. pp. 343–430, large 4to., with lithogr. pl. 45–67 (all remarkably beautiful). Published by the K. K. Geol. Reichsanstalt, Vienna.

PROC. BOSTON SOC. NAT. HIST., vol. xi.—Page 289, Odonat-fauna of Cuba; *H. Hagen.*—p. 295, Odonata of Hayti; *P. R. Uhler.*—p. 301, 303, Shell heaps of Goose Island; *Wyman, Morse.*—p. 302, Navicula Carassius; *C. Stodder.*—p. 304, Antiquity of Man; *L. Agassiz.*—p. 306, On the stridulation of some New England Orthoptera; *S. H. Scudder.*—p. 313, On symmetry in leaves, &c.; *B. G. Wilder.*—p. 317, Skulls of American bison and European aurochs; *L. Agassiz.*—p. 369, Relation between the plumage of birds and modes of nidification; *S. Kneeland.*—p. 321, Form of Volcanic Craters; *W. T. Brigham.*—p. 323, Mode of locomotion in Chelifer; *H. Hagen.*—pp. 327, 403, on the island of Yesso and the Ainos; *A. S. Bickmore.*—p. 337, Animals of shell-heaps of New England; *J. Wyman*—p. 341, On the Red Sandstone of Vermont and its relations; *B. Perry.*—p. 355, Structure, flight, and habits of the varieties of the domesticated Pigeon; *E. D. Harris.*—p. 361, On a point in the habits of the Diatomaceæ, and Desmidiaceæ; *A. M. Edwards.*—p. 365, Development of a Dragon-fly (Diplax); *A. S. Packard.*—p. 372, On Lachlania abnormis, of the Ephemerina; *H. Hagen.*—p. 375, Supplement to a list of the Butterflies of New England; *S. H. Scudder.*—p. 384, Classification of the Mole crickets; *id.*—p. 387, Two species of salt fly; *A. S. Packard.*—p. 390, Relative rank of families of Orthoptera; *S. H. Scudder.*—p. 391, Journey through China, &c.; *A. S. Bickmore.*—p. 393, Structure of the ovipositor, and homologous parts in the male insect; *A. S Packard.*—p. 399, Wingless white Ant from Japan; *H. Hagen.*—Fossil Insects from the coal formation of Illinois and Ohio; *Scudder.*—p. 404, Limnadia Americana; *E. S. Morse.*—p. 405, on the Angora Goat; *Hayes.*—p. 433, Meteor seen in Prussia; *H. Hagen.*—p. 435, American Chelifers; *id.*—New Butterfly (Theca) from Florida; *Scudder.*—p. 439, Diatoms, &c., from soundings off the coast of Maine; *C. Stodder.*—p. 440, Observations on Crania; *J. Wyman.*

MEMOIRS OF THE BOST. SOC. NAT. HIST. 4to. Vol. 1, part iii.—p. 305, On the Spongiæ ciliatæ as Infusoria flagellata, or observations on the structure, animality and relationship of *Leucosolenia botryoides* Bowerbank, with 2 pl.; *H. J. Clark.*—p. 341, Notes on the volcanic phenomena of the Hawaiian Islands, with a description of the modern eruptions, with 5 pl.; *W. T. Brigham.*

PROC. ACAD. NAT. SCI. PHILADELPHIA. 1868, No 1.—page 2, Observations on some specimens of Vertebrata from Nevada and Lower California; *E. Coues.*—Monograph of the Alcidæ, *id.*—p. 81, List of birds from S. Arizona; *id.*—p. 86, Descriptions of new species of diurnal Lepidoptera, series iii; *T. Reakirt.*


38

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

Art. XIV.—*A new Theory of Vision;* by Samuel Rowley, M.A.

My purpose in the following communication is to offer a theory of vision which I believe has not been heretofore advanced, and which I venture also to believe to be the true one. But before entering upon its consideration it may be proper briefly to state, with reasons for rejecting them, the theories hitherto advanced.

The theory of Aguilonius supposes that all the images, impressed on the retinæ at any given instant, are seen in a plane perpendicular to the plane of the optic axes at their point of convergence, and parallel to the line joining the centers of the two pupils; and consequently that the two images of any object situated in this plane, occupying as they necessarily do one and the same place, are seen single with two eyes.

If any person whom a little practice has fitted to make such experiments, will place at some short distance, say three inches, in a line parallel to the eyes, two small objects, one directly before the central point of the interval between the eyes, and the other at a distance from this of say ¾ of an inch, and direct his optic axes to the former, he will not see the two images of the latter as one, though their object is situated in the plane mentioned, but as two distinct images, the one beyond the other.

Experiments to the same effect might easily be multiplied.

But the one given abundantly demonstrates the inaccuracy of the theory of Aguilonius.

The theory of *corresponding points*, so called (as held on physiological grounds by Galen, Alhazen and Newton,) cannot be denominated a theory of vision. It is directed to the accounting for but one of the phenomena of vision—that of single sight with two eyes. It does not deal with visible direction and distance; whereas these conditions, governing, as they do, external visible position, include the result of single sight with two eyes as a necessary consequence.

The theory of vision propounded by Dr. Smith in his Optics, and that by Dr. Wells, from which he derives his explanation of the phenomenon of single vision with two eyes, in his "Essay on single vision with two eyes," are practically (as I trust will satisfactorily appear hereafter) merely attempts to formulate the misapprehensions by consciousness of true visible distance which take place under the influence of habit. A simple experiment subversive of both these theories may be made thus. Let two small objects, as pins, in a board, be placed directly in front of the middle point between the eyes, the one at some short distance, say six inches, and the other at some more considerable distance, say eighteen inches, and let other two small objects be placed on opposite sides of the near one in lines between the eyes and the far object, and the optic axes be directed to the near object. Then the two images of the remote object will be found at no greater distance from each other than the images of the objects at the sides of the near one, or all will be apprehended as lying in the figure of a rectangle.

But by the theory of Dr. Wells* the two images of the remote object (since this object lies in the direction which he has named the common axis) are seen in the optic axes—the right eye's image in the left eye's axis, and *vice versa*—and at a distance from each other (if located at the same distance from the eyes as the object)† of $4\frac{8}{10}$ inches, whereas the images of the two lateral near objects, since these occupy the line named *horopter*, will be seen in the places of the objects at a distance from each other of $1\frac{6}{10}$ inch.‡

As to the two images of any object more or less distant than the convergence of the axes, the theory of Smith places them in lines drawn from the eyes to the object somewhere between the place of the object and the point of convergence, "but not very far from the real place," i. e. the place of the object.§ Hence, while the two images of the object on either

* Essay upon Single Vision with Two Eyes, p. 38. London, 1818.
† Ib. page 27. ‡ Ib. pages 5 and 46.
§ Smith's complete System of Optics, Art. 137.

side of the point of convergence will be seen coincident with their object at a distance of $1\frac{6}{10}$ inch from the two images (seen in like manner) of the object on the other side, the two images of the remote object will be seen at a much less distance from each other, or, following the proportions of the construction given us by that author, at a distance of $\frac{5}{10}$ or $\frac{6}{10}$ inch.

Aguilonius is the only author who, meeting the question in its full extent, undertakes to assign for each image both the line of visible direction and the absolute visible distance from the retina on the line of visible direction.

Sir David Brewster indeed gives both conditions of external visible position for the images falling *on the vertices of the retinæ.* But for every other image he merely assigns Kepler's line of visible direction, being silent regarding the visible distance—the essential complement of the former in determining visible position.

I will now propose and proceed to put to the test of experiment, what I believe to be the true theory of vision; premising the remark that in establishing its existence it will not render the proof offered less exact if I do not undertake to determine the precise point where a given ray* strikes the retina nor the precise point at which the line of visible direction crosses the optic axis, provided we admit, (what I think will not be disputed) that where the angles with the optic axes of two rays, falling one on each cornea, are equal, the distances of the images on the retinæ from the extremities of the axes are equal, and provided we admit, (what I also think will not be disputed) that where the angle with the axis of one of the rays is but *very slightly* increased, the angle formed after decussation is not appreciably *un*equally increased; and further premising that I will neglect any effect of refraction and also suppose a coincidence of the line of the ray with the line of visible direction.

All the component points of the total impression on the retina of either eye, before becoming cognizable mental phenomena, are simultaneously and in lines drawn from them through a point situated, say a little behind the crystalline lens, referred outward to a surface, parallel to the impressed surface of the retina conceived to be simply expanded coextensively with the impression as expanded by outward reference and cutting at right angles the optic axis of that eye at the point where it intersects the optic axis of the other eye.

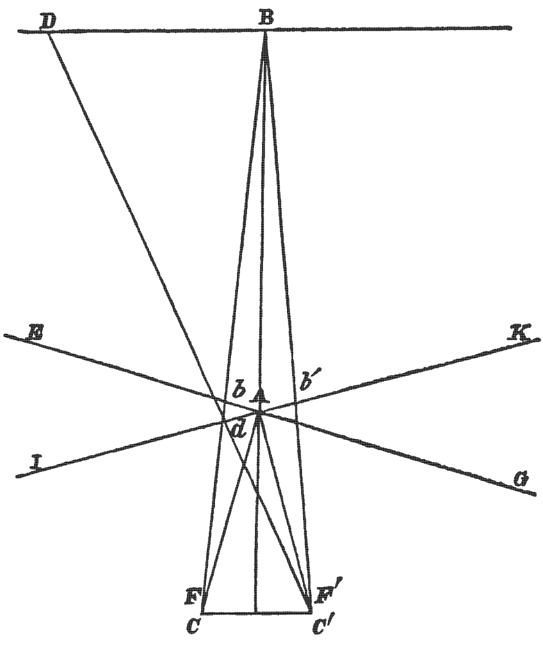
* Here and wherever in this paper the word *ray* is used, it is intended to signify the central or axial ray of the pencil.

Or, to change the form of statement, *the entire impressions on the retinæ, before becoming objects of consciousness are projected in space upon surfaces bisecting each other (at an angle greater or less according to the distance) in a plane perpendicular to the plane of the axes—the component points of each impression being simultaneously referred outward in lines passing from them through a point a little behind the center of the crystalline lens, but, excepting the expansion and the inversion resulting from the crossing in the eye of the directions of outward reference, undergoing no change of relative position—the distance between the planes passing at right angles to the optic axis through any two of the successive concentric zones of points, which make up the retinal impression, continuing the same.*

I do not mean here to assert that an outward projection actually takes place, but that the *effect* of such a projection, produced by an intermediate process of mind, is presented to the consciousness.

On a suitable plane surface take two points C, C′ (fig. 1),

1.



$2\frac{4}{16}$ inches apart, and in a line bisecting at right angles one joining these points take other two points A, B, distant respectively from the point of bisection six and eighteen inches.

Draw the straight lines C A, C′A, and perpendicular to them at A the two lines EG, IK. Through the points F, F′, taken in the lines CA, C′A, $\frac{1}{16}$ inch from C, C′, draw the lines FB, F′B, intersecting EG, IK, at *b*, *b*′. Through the points F′ and *d*,—a point in IK where a line passing through *b* perpendicular to CC′ intersects it—draw a line. In this line, where a line passing through B parallel to C C′ intersects it, take the point D. The distance of this point from B will be $4\frac{9}{16}$ inches.

Now suppose the points C, C′, to represent the centers of the eyes; points F, F′, the focal centers; A, B, D, the situations of luminous points; the lines CA, C′A, the optic axes produced; the lines EG, IK, lines of intersection where two surfaces slightly convex toward the eyes, perpendicular respectively to the optic axes at their point of meeting, cut the plane of the axes; BF, BF′, lines of luminous rays passing from the point B through the points F, F′; DF′ the line of ray passing from D through F′ to the retina; *b* the point where the line BF intersects the line EG; and *d*, the point of intersection of DF′ with IK. Then, if the above ennunciated proposition be true, to wit: that an impression made by a luminous point on the retina of the right or left eye becomes visible and is seen where a line drawn from it through a point a little behind the center of the crystalline lens pierces a slightly convex surface perpendicular to the optic axis of that eye at the place of its meeting with the axis of the other eye, the image of the point at B, will be seen from the left eye at *b*, and the image of the point at D from the right eye at *d*, both in the same perpendicular of no variation,* or the image of the point at B will be seen by the left eye a little behind the place in which the image of the point at D will be seen by the right eye.

The experiment may be made thus. Take a thin rule, (as a plane scale) twelve inches in length with the right hand, and another rule with the left hand. Hold the rule in the right hand with its wide sides perpendicular to the plane of the horizon, in a line running away from the middle point between the eyes so that the near end shall be distant six inches and the remote end rest upon a horizontal bar of a well lighted window at right angles to the rule, and parallel to the eyes.

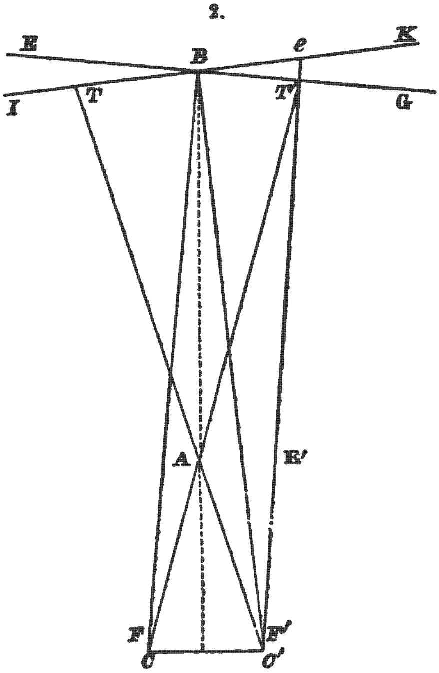
To obtain the former distance with accuracy it is necessary to have two small objects, as two pins, placed equidistantly on opposite sides of the point of intersection of the bar by the rule, distant from each other $4\frac{8}{16}$ inches, and when these shall be seen across (or lie in the axes exactly fixed upon)

* By perpendicular of no variation is meant a line perpendicular to the base of vision, for the definition of which see note, p. 164.

the near end of the rule, that distance for eyes $2\frac{4}{15}$ inches apart will be accurately found. Now fix the axes of the eyes on the near end. The images of the remote end will appear as two at the sides, that of the left eye at some unknown distance on the left, and that of the right eye in like manner on the right. Holding the rule in the left hand in the same position with respect to the plane of the horizon as the other, apply its remote end on the window bar to the left of the remote end of the rule in the right hand, so that the right eye's image of the former will cover the left eye's image of the latter. Now measure the distance between the far ends on the bar. If all the conditions of the experiment shall have been exactly complied with the distance found will be $4\frac{9}{15}$ inches, as predetermined in the diagram.

The experiment, then, shows that the geometrical consequence of the principles of the proposition, as illustrated in the diagram, holds good in actual practice, and that when the axes are fixed upon the images of an object placed as a luminous point at A, the left eye's visible image of an object placed as a point at B in the line FB, is seen a little beyond where the right eye's visible image of an object placed as a point at D in the line drawn through F′ and *d* is seen, both in the same line of no variation. It is obvious by the proposition that it ought not to affect the "conjunction" of the images, wheresoever in the line passing through F′ and *d* the object might be placed; and this will be found to be the fact. It was placed opposite B at D merely for convenience of applying the diagram in actual experiment, and to procure a correct appreciation of the interval between the images.

2.



Suppose in the second place, the axes represented by CB, C′B, fig. 2. Then by my proposition the left eye's visible image of the

luminous point at A should be seen at T′ in a surface cutting the axis FB at right angles in B (and the plane of the axes in the line EG) and the right eye's image at T.

But if the left eye's image of the point at A is seen at T′ then by the same proposition the right eye's image of the point at E′ opposite A in a line between F′ and *e*, a position in IK, directly beyond T′, should be seen in the same line of no variation a little beyond T′ at *e*. If the experiment be made it will be found that the right eye's visible image of an object placed as a point at E′, nearly $1\frac{6}{10}$ inch from A, will be seen a little behind the place where the left eye's image of an object placed as a point at A is seen.

So the two middle images of the pins placed as a test of distance in the first of these experiments are seen united at the intersection of the surfaces of vision.

If the surfaces of vision were planes, since their line of intersection would then coincide with the line of intersection of the vertical planes of the axes, it is evident that the images of these pins longitudinally bisected by the latter planes would exactly coincide. But owing to the slight curvature of the surfaces of vision, the line of intersection of these surfaces immediately begins very gradually to recede from that of the vertical planes of the axes, and the longitudinal axes of the images of the pins to cross over and fall on opposite sides, thereby preventing a mathematical coincidence of the images.

This effect of the departure of the intersection of the surfaces of vision (supposing the optic axes parallel to the horizon, to which the pins are perpendicular, to be directed to the bottoms of the images) may be counteracted and a mathematical coincidence of the images nearly produced by inclining the pin whose image is seen by the left eye a very little to the left and the other duly to the right. Should this small inclination be given with such a curve that a line drawn from any point of the axis of the pin to the focal center of the eye would intersect the line of intersection of the surfaces of vision, then an exact mathematical coincidence of the images would be secured.

The approximate result may be obtained if two rulers* or the legs of a pair of compasses† be so placed that their longitudinal axes shall lie in the vertical planes of the optical axes.

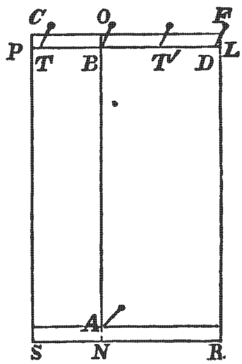
It is evident that for the two objects in the vertical planes of the axes, we may put a single object at the intersection of these planes.‡

* See Wells, p. 36. † Smith's Optics, Art. 977. ‡ Wells, p. 49.

But to make the accurate performance of these experiments easy, some special instrument must be provided. A piece of pine board whose opposite great surfaces are rectangles parallel to one another, thirteen inches long and eight inches wide, with five pins, furnishes the means for obtaining accurate results, where the distance between the near and far points does not exceed twelve inches.*

To use this contrivance—supposing the distance from the middle of the line between the centers of the eyes to the near objective point to be six inches, and from the near to the far objective point twelve inches—in the surface SRFC (fig. 3) $\frac{1}{2}$ inch from the short side SR, and five inches from the long side RF, take the point A. Through this point and at right angles to the side SR, draw a line NO. In this line, twelve inches from the point A, take the point B. Through this point, and at right angles to this line, draw another line PL. In this latter line, take the two points T, T′, each distant $2\frac{4}{16}$ inches from the point B, and $4\frac{8}{16}$ inches from the other; and finally $4\frac{9}{16}$ inches from the point B, take the point D. At each of the points, so taken, set a pin. Now place the board in a horizontal position a little below and parallel to the plane of the axes, with the pin at A six inches from the middle of the line joining the centers of the eyes, and, together with the pin at B, in the vertical plane which bisects this line at right angles; which disposition of the pins is had, when, looking at the images of the pin at A, the left eye's image of the pin at T′, and the right eye's image of that at T, are found lying in the axes.

3.



When now the axes are fixed upon the images of the pin at A, the right eye's image of the pin at B will be seen in the same line of no variation as the left eye's of the one at D, and at a point a little beyond where the latter is seen; and when they are fixed upon the images of the pin at B, the left eye's image of that at A will be seen in a position a little before where the right eye's image of a pin, set nearly $1\frac{6}{16}$ inch directly to the right of A, will be seen, the distance between these being sensibly less than that between the images received from B and D, when the axes were converged toward A.

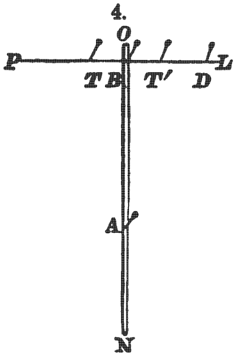
* Another board of the same width and 18 inches long serving as a support to this, to be held with one end against the nose by the hands pressing both pieces at the sides, will be found serviceable for maintaining steadiness of position.

A still better instrument, is a small wooden cross consisting of a slender bar and sliding transverse piece, which may be any slip of wood, (as a piece of common lath) notched at the center so as to receive the longitudinal bar in its full thickness at right angles, and also capable of being shifted and fixed by wedging at any point.

For all experiments in which the distance of the remoter of the two stations designated A and B, figs. 1 and 2, is not greater than 18 inches, the length of the bar should be 18 inches, and that of the transverse piece, 12 inches.

To apply this instrument instead of the window bar and rulers in the first of the examples taken, fix the transverse piece at the distance of 17 inches from one end of the shaft, and laying the cross on a table with its level side upward, draw on the upper side of the shaft lengthwise the bisecting line NO, and lengthwise on the upper side of the transverse piece, the bisecting line PL. At B, the point of intersection of these lines, set a pin, and at the point A, 12 inches from the point B, in the line NO, also one. At the points T, T′, in the line PL, each distant $2\frac{4}{10}$ inches from B, set two other pins, and at D, $4\frac{9}{10}$ inches from B, another. Then placing the cross so as to lie with the extremity N, of the shaft against the upper part of the nose, and its upper surface a little below and parallel to the plane of the axes, and keeping the head fixed, look with each eye in rapid succession at its image of the pin at A. If the right eye's image of the pin at T, and the left eye's of that at T′, be seen in these directions of the axes, the pins at A and B are 6 and 18 inches distant from the middle point of the line joining the centers of the eyes. But if they should fall within or without the axes thus directed, then the transverse piece, together with the pins at A and B, 12 inches apart, must be shifted slightly forward or backward, until the requisite position is found.

4.



Holding this instrument in the same position with respect to the eyes as that just above given to it for ascertaining distance, turn it toward a strongly illuminated white wall or sheet of white paper or (which is better) the clear sky. If now the axes be fixed upon the images of the pin at A, the right eye's image of the pin at B, and the left eye's of that at D, will be seen lying in the same line of no variation, the former a little beyond the latter. But if having transferred the transverse piece to the

distance of A, the axes be fixed upon the images of the pin at B, then the image of the pin at A, seen by the left eye, and that of a pin placed nearly $1\frac{6}{16}$ inch directly to the right of A, seen by the right eye, will be found lying in the same perpendicular of no variation, the latter beyond the former, the distance between the two images being less than in the first case.

In experimenting with this instrument, the position of the image of the pin at B seen by the left eye in a surface at right angles to the optic axis of that eye is visibly realized. For when the axes are fixed toward A (as in the first instance) the image of the transverse piece seen from the left eye visibly has a position at right angles to the axis of that eye, and intersects the corresponding image of the right eye, in a line of common intersection crossing each image where that image, whether referred back to the retina or to the distance of the transverse piece itself, would be pierced by the optic axis.

In making these experiments, care should be taken not to turn the axes into the oblique directions, to do which there is always a strong inclination; for then the images, which were seen, in those directions as described, the one before or behind the other, would be seen coincident.

I will now proceed to answer an apparent objection. In the first demonstration, under my proposition, I undertake to show that the impression on the left eye from B becomes visible and is seen at *b*, by showing that it is seen a little behind where the impression on the right eye from an object placed at D is seen by the right eye, arguing that nothing else than a projection in the manner described can account for the local relationship in which these impressions are seen. The evidence of consciousness, then, that these impressions are seen in a certain local relationship, is made the groundwork of my argument. Now, it may be urged that the evidence of consciousness is equally strong, that the impression from the object at B on the left eye, when seen, lies in the axis of the right eye at the actual distance of the object itself at B.*

In the first place, let us determine whether consciousness, when carefully examined, does really continue to find this impression elsewhere than at *b*. To reveal in consciousness the true distance in a lateral direction of the visible image at *b* of an object at B (fig. 1), employing the pins and board, place a pin at *b*. The left eye's image of the pin at B will not be recognized to the left of the left eye's image of it.

Again, take three equal, small rings of pasteboard. Attach to each, in the direction of a radius produced, a pin, by cement-

* See Wells, pp. 38 and 27.

ing with a drop of sealing wax the head of the pin to the edge of the ring.

At B, fig. 3, (the board duly widened,) a point $4\frac{8}{10}$ inches to the right of B, and a point $4\frac{8}{10}$ inches to the left, station these pins, so that the rings shall be parallel to the eyes of the observer. At the points A, T, T′, set simple pins. Now on converging the axes toward A, till the right eye's image of the pin at T and the left eye's of the pin at T′, are hidden in the combined images of the pin at A, the images of the rings will be apprehended in their true visible places.

To make another experiment of the kind to which the last belongs, on a glass of a window situated at a convenient height, stick a number of wafers, placing their centers in a horizontal line and separating the adjacent centers by a distance less than that between the centers of the eyes, but being extremely careful to make all these distances exactly equal. Direct the axes through the centers of two adjoining wafers, each axis through that center lying on its own side, and presently all the visible images (except usually the left terminal image of the right eye's series and the right terminal one of the left eye's), will be recognized by consciousness in their true visible positions.

If the adjoining centers of the wafers are very close to each other (as a little over $\frac{1}{4}$ inch, supposing the wafers to be $\frac{1}{4}$ inch in diameter) and the eyes at a short distance, say 4 inches, *the curvature of the surface of vision will be plainly appreciated*, and if the plane of the axes be turned on the line joining the intersection of the axes to the center of the interval between the eyes from a coincidence to an angle with the horizontal plane passing through the centers of the wafers, *the curvature of each eye's series will be separately appreciated.*

An experiment of the same kind may also be made by means of the similar figures lying at equal distances in the surface of a papered wall.

If the experimenter will station himself facing such a wall and converge his optic axes, so that the axis of his left eye shall pierce the center of some figure in front of him and that of his right eye the center of the next one to the left of the other, then shortly each eye's image of the papered wall will, to his consciousness, take its true visible position at the distance of the convergence of the axes—the images of the figures appearing smaller or larger according as the distance of the point of convergence is less or greater. A variation (from the standard) of the distance between the centers of the figures of the wall, will produce a seeming elevation or depression of the visible images. The cause of this last as well as that of the principal phenomenon, will be apparent, when it shall have been

explained why the lateral images of an object which is at a greater or less distance than that of the convergence of the axes, are not apprehended in their true visible positions in the surfaces of vision.

Why then does consciousness mistake, precisely as we find it doing, the true place of the visible image of any object, lying not at the point of intersection of the axes nor at any point equally distant with this from the base of vision ?*

My answer is, that under tendencies resulting from experience, consciousness apprehends erroneously the distances really presented for its action according to the laws of vision, and with respect to each lateral image apprehends the distance of the surface of vision from the base of vision, and the distance in the surface from the image at the center for the distances that would exist between these surfaces and these images, if the given image, now laterally and obscurely seen, were with its corresponding image of the other eye directly and distinctly examined at the center.

Hence the place in which such an image seems to be seen.

For example, let the axes be turned to the images of an object at B, fig. 2. They will be seen at B, the left eye's $2\frac{4}{10}$ inches, to the left, from its image at T′ of the object at A, and the right eye's $2\frac{4}{10}$ inches, to the right, from its image at T of the object at A, each in a surface of vision, measuring from the apex, 18 inches from the base of vision.

Let now the axes be turned to the images of the object at A, fig. 1. These will now be seen at A, that of the left eye's $\frac{8}{10}$ inch to the right, from its image at *b* of the object at B, and the right eye's $\frac{8}{10}$ inch to the left from its image at *b′* of the object at B, each in a surface, measuring from the apex, 6 inches from the base of vision.

At first then, the left eye's image of an object at B when particularly examined, will always be apprehended at the distance of $2\frac{4}{10}$ inches, to the left, from the image at T′ of an object at A in the surface of vision 18 inches from the base.

Hence after due experience will arise a tendency of mind to apprehend this image at a distance of $2\frac{4}{10}$ inches, to the left, from the image of the object at A in the surface of vision 18 inches from the base. Therefore, when the axes are turned toward the images of the object at A, the mind will be influenced by this tendency, with respect to the left eye's image at *b* of the object at B, to apprehend 6 inches, the distance of the surface of vision from the base, as 18 inches, and $\frac{8}{10}$ inch, the

* By *base of vision* I mean a plane cutting perpendicularly at the center of the interval between the eyes, a line drawn through this point and the point of convergence of the optic axes.

distance in the surface from the image at A of the object at A, as $2\frac{4}{10}$ inches.

Again, the effect of experience in regarding directly the images of the object at A will be a habit whereby, when the axes are directed to the images of the object at B, the mind will be led to mistake $2\frac{4}{10}$ inches—the distance in the surface of the left eye's image at T′ of the object at A to the right of its image at B of the object at B—for $\frac{8}{10}$ inch, and 18 inches—the distance of the surface of vision from the base—for 6 inches; and $2\frac{4}{10}$ inches—the distance in the surface of the right eye's image at T of the object at A to the left of its image at B of the object at B—for $\frac{8}{10}$ inch, and 18 inches—the distance of the surface of vision from the base for 6 inches.

Thus since the more remote parts of a solid object will seem to be seen at equally more remote distances in the image, and the entire image at a distance equal to that of the object, the image will seem to be seen with the same form and magnitude as really pertain to the object.*

We are now also prepared to explain the cases in which the visible image, though the object be situated at a greater or less distance from the base of vision than the point of convergence of the axes, is accepted by consciousness in its true place, as in the experiments with the equidistant pins, with the wafers upon glass, and the figures in the papered wall.

In these cases it will be perceived that any two images seen as one, though proceeding from two different objects, have the same positions in the surfaces of vision as they would if they proceeded from a single object placed at the intersection of the lines drawn from the two different objects to the eyes.

Further, so great is the propensity to appreciate singly, that any slight discrepancy between these images and those produced by one and the same object will not prevent an acceptance of a correspondence, and, since all the equivalent single objects would lie at the same distance, no habit can exist to influence the mind with respect to any images to locate the surfaces of vision at a greater or less distance from the base of vision than the true. Hence all the images will be apprehended in their true visible places. But if the interval between any two objects of such a series should differ from the other intervals, then for the two (of the four images presented) which are accepted as corresponding, or capable of being seen singly in direct vision, if these should be lateral, or if central, then for all the rest, from the necessary principle of habit above laid down, the surfaces

* In ordinary lateral vision, (i. e. vision had at the sides of the line of intersection of the surfaces of vision), two corresponding images or the corresponding parts of two complex images at the concurrence of the axes are appreciated singly by reason of inattention to, or neglect of, one of them.

of vision, measured from the common center, will seem to exist at such a distance from the base of vision as that at which they would it these images were directly seen in the axes.

In this experiment with the papered wall, with every movement of the head a movement of all the images in the same direction takes place, because with the eyes fixed toward any two figures, a movement of the head in any direction will carry in the same direction all the points of intersection of the lines of direction and consequently all the images in the two surfaces. So when the axes are made to pass through any two of a series of equal and equidistant objects to a point beyond, as in the case of the experiment with the wafers upon glass, a movement of the head in one direction will produce a movement of the images in a contrary direction.

There are other circumstances incident to a lateral image in the surface of vision, besides distance from its twin, which will produce habits operating to defeat a correct appreciation of the two distances.

Such a circumstance is the visible magnitude.

Thus, whatever the visible magnitude of an aggregate lateral image, which has been often seen in direct vision, this magnitude will be taken for that found on particular examination, and the visible distance for that corresponding to that magnitude. But since lateral images in the surface of vision, not distinguishable from each other by consciousness may result from objects of very different magnitudes placed at suitably different distances, a given magnitude of image will not always excite the same tendency of mind.

To illustrate this, take the board with pins placed at A and B (fig. 3,) depress the far end till the surface is hidden by the near end, and, stopping the light flowing from the near pin to one of the eyes by interposing near the eye an adequate object, fix the axes of both toward the remote pin. As soon as the eye has ceased its alternations of adjustment (which have been performed so rapidly as to keep up a sensibly sustained distinctness of both images), and no longer experiences the sensation of focal adjustment to the near pin (which has excited a tendency of mind to mistake the image at the distance of the object), the now shadowy image of this pin will take its position beside that of the far-pin. This it will generally do slowly, because the image in the surface might have resulted from a pin of due dimensions situated at every intermediate point, but in continuous succession giving up every intermediate position it will finally come to rest in its true visible place. Occasionally owing to the dominancy of some habit, it will seem to take a place at a greater or less distance than the true.

But habits created by magnitude may cause mistakes of visible distance involving also *the image in the axis.*

Thus for example, if the mind has been accustomed to appreciate the images of mountains of moderate magnitudes, thereby tendencies will be impressed upon it to appreciate moderate dimensions with images of this class. Hence when the image of a mountain of extraordinary magnitude is presented (in an ordinary state of the atmosphere), consciousness will be influenced to appreciate the dimensions as less than they really are, and, in order to this, the distance from the base of vision as a duly less one. And the effect on any concomitant images, as of trees or animals, would be a like seeming diminution of their visible magnitudes. For it is plain that if consciousness makes a mistake of distance, then in obedience to the law of habit it will assign that size which the image would really have at that distance, or that effect of lateral distance, or resulting expansion, which the divergence of the lines of direction would give at that distance.

Dr. Abercrombie relates the following instance as having occurred in his own experience. "I remember," he says, "once having occasion to pass along Ludgate Hill, when the great door of St. Paul's was open, and several persons were standing in it. They appeared to be very little children; but, on coming up to them, were found to be full grown persons."

The theory which I have thus advanced, taken in connection with the mistaking practice of consciousness, will, I believe, furnish a satisfactory explanation of all the phenomena of vision—some of the more interesting of which phenomena, as well as the subject of monocular vision, I propose in a future paper to consider.

Hastings-upon-Hudson, N. Y.

Art. XV.—*Fundamental Principles of Molecular Physics;* by Professor W. A. Norton.

In a recent work by Joseph Bayma, S. J. Professor of Philosophy, Stonyhurst College, England, in which a new theory of Molecular Mechanics is ably set forth, I find a brief critique of my theory of Molecular Physics, published, originally, in this Journal, and republished in the London and Edinburgh Philosophical Magazine. To this I propose to reply, and at the same time to remark incidentally upon some of the fundamental principles of the theory advanced by Professor Bayma. This can be most briefly and effectively done by taking up the

different objections urged by him in due order, and commenting upon them in succession;—having a care not to dissociate remarks that should properly be presented together. The quotations made will be indicated to the eye by being printed in smaller type.

A great number of scientific men, to give an explanation of calorific, electric, and luminous phenomena, assume that aether pervades all ponderable bodies; whence many of them have come to the conclusion, that every molecule of a body is surrounded by an æthereal atmosphere, the action of which is considered to be the source of those phenomena. Professor W. A. Norton, in a series of interesting articles published in the American Journal, gives a theory of molecular physics, of which the fundamental principle is, that each molecule is formed by an atom of ponderable matter surrounded by two aethereal atmospheres of a different kind. I give his words.

The established truths and generally received ideas which form the basis of the theory, are as follows:

1st. All the phenomena of material nature result from the action of force upon matter.

2nd. All the forces in operation in nature are traceable to two primary forces, viz: attraction and repulsion.

3rd. All bodies of matter consist of separate indivisible parts, called atoms, each of which is conceived to be spherical in form.

4th. Matter exists in three forms essentially different from each other. These are 1st, ordinary or gross matter, of which all bodies of matter directly detected by our senses either wholly or chiefly consist. 2nd. A subtile fluid, or ether, associated with ordinary matter, by the intervention of which all electrical phenomena originate, or are produced. This *electric ether*, as it may be termed, is attracted by ordinary matter, while its individual atoms repel each other. 3rd. A still more subtile form of ether, which pervades all space and the interstices between the atoms of bodies. This is the medium by which light is propagated, and is called the *luminiferous ether*, or the *universal ether*. The atoms, or "atomettes" of this ether, mutually repel each other; and it is attracted by ordinary matter, and is consequently more dense in the interior of bodies than in free space.

5th. Heat, in all its recognized actions on matter, manifests itself as a force of repulsion.

The corner stone of a physical theory of molecular phenomena must consist in the conception that is formed of the essential constitution of a single molecule; understanding by a molecule an atom of ordinary matter, endued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion upon other molecules. In seeking for this, the most philosophical course that can be pursued is to follow out to their legitimate conclusions the general principles already laid down. . . . . The conception here formed of a modecule involves the

idea of the operation of the two forces of attraction and repulsion: a force of attraction is exerted by the atom upon each of the two atmospheres surrounding it; and a force of mutual repulsion between the atoms of each atmosphere. These we regard as the *primary forces* of nature, from which all known forces are derived.

These are the capital points of Prof. Norton's ingenious theory. But we think that such a theory contains a great deal of arbitrary assumption. And indeed on what evidence are we to grant that matter exists in three forms essentially different from each other? Then how can we know the existence of atoms of gross matter having a spherical form, and therefore extended though indivisible? Why should we admit two æthereal fluids, which are both repulsive and only differ in subtlety? All this the learned Professor assumes without proof, apparently because it consists of "established truths and generally received ideas." But we say that no one has up to this day established the truth of such propositions. As for "received ideas," every one knows how often questionable notions have been and are received without serious examination, especially when expressed by Professors in a very dogmatic style. Are not a thousand hypotheses received? and do they cease to be hypotheses, although he who makes use of them for building a theory adorns them with the high name of principles.

This is all very plausible, but the objections urged are destitute of any real force. We will first consider the general intimation that the theory rests upon "a great deal of arbitrary assumption." No theory of Molecular Physics can, in the nature of things, have any other foundation than general principles to be regarded as hypotheses that have been rendered more or less probable, either by inductions from observation or by à priori reasonings. Molecular Physics cannot be erected, like Mathematics, upon a foundation known from the first to be eternally sure, that of self-evident truths. Mechanical axioms may exist as mere figments of the mind, and have often risen like bubbles in the minds of speculative philosophers, shone with an evanescent splendor, and suddenly burst at the touch of a hard fact. Our author is another instance of a learned philosopher who has faith in such unsubstantialities, and thinks to substitute them as a proper basis for a theory of Molecular Mechanics, in place of the general conceptions to which the progress of science leads, and by which alone its highest inductions find any explanation—regards the latter as arbitrary assumptions, and his own mental convictions of what matter must be and how it must act, as the only reliable foundation upon which to build. It is true that he takes exception to Principles 3rd and 4th, from the inductive point of view. Upon this ground—the only legitimate one to be occupied—I

am quite ready to meet him: but I wish to enter here, at the outset, a demurrer against the virtual claim of the superiority of his own à priori method of establishing his fundamental principles. Such a claim is implied in the intimation that "no one has up to this day established the truth of such propositions;"—as will be best appreciated by those who have read Prof. Bayma's book. Having proved, as he conceives, his propositions, and clinched each one of them with a Q. E. D., he insists that obvious intimations of Nature are to be discarded because the stamp of infallibility cannot be put upon them at once, before the test of availability in the explanation of phenomena has been applied. It was evident from the tenor of my exposition of the subject that the "established truths" referred to were merely regarded as having been virtually established, or rendered highly probable, by the inductions of science. The claim implied in Prof. Bayma's criticism, that they require a higher confirmation, in fact a demonstration of their truth, is not to be admitted.

He asks:

> On what evidence are we to grant that matter exists in three forms essentially different from each other?

A sufficient answer to the critic himself, is that pursuing a systematic course of deduction from his leading principles, and his assumptions of the essential nature of matter, he actually proves to his own satisfaction that matter does in fact exist in essentially three different forms. He reaches the conclusion that every primitive molecule consists of an attractive nucleus surrounded by a repulsive envelop. My own position is that every primitive molecule consists of an attractive atom of gross matter surrounded by a repulsive atmosphere of electric ether. The atom of gross matter answers to his attractive nucleus, the electric ether to his repulsive envelop. The difference of doctrine, from the present point of view, is in name only. In another connection he elaborately undertakes to prove that ether (i. e. the ether of space) is a "special substance." Thus he makes out that there are three essentially different forms of matter.

But to reply to others who may be disposed to adopt the objection urged. No one will deny the existence of gross or ponderable matter; or of something which has all the mechanical attributes of matter. That an ether exists in space and within transparent media, we may certainly regard as abundantly established by optical phenomena. As to the electric ether, the evidence of its existence is that the great body of electric and magnetic phenomena, it is generally

conceded, admit of satisfactory explanation on the hypothesis of an electric fluid, or ether, intimately associated with matter; and that no successful attempt has yet been made to account for the simplest of these phenomena on any other hypothesis. Some physicists, it is true, are striving to do away with the supposed electric fluid;—prompted by the conjecture that Nature must operate by some simpler method, and work out all her wonderful diversity of phenomena by one, or at most, two forms of matter. Shall we wait until these physicists have realized their aspirations, at their discouraging rate of progress; or, guided by the indications of Nature, strive to link all natural phenomena together by a few recognized principles. A theory that shall accomplish this is the great desideratum. Even should such a theory not rest upon the highest and fewest possible mechanical principles, still the generalizations embodied in it must have their counterparts in certain physical truths, to the knowledge of which it will be likely to lead. It is by following the ascending grade of generalizations that speculative science has hitherto progressed. Preconceived notions of what matter must be in its essential nature, or by what form of matter, or varieties of method, Nature must operate have thus far contributed little to its advancement: and in fact when we consider that we positively know and can know nothing, à priori, with regard to the essential nature and condition of matter, and its means and mode of operation, such notions are entitled to little credit.

Our author implies in the remarks above quoted that the existence of an electric ether is not only not an "established truth," but is to be ranked among those questionable notions that have been received without serious examination. This implication is obviously unjust. Besides, the serious examination that he has given the subject only leads him to confirm the substantial truth of what he would here seem to discredit—for, as we have already seen, his "repulsive envelope" is essentially my "electric atmosphere."

Why should we admit two æthereal fluids which are both repulsive and only differ in subtlety.

Prof. Bayma and myself agree in admitting the existence of two *kinds* of matter,—attractive and repulsive; and as we have seen, three *forms* of matter. Is it inherently any less probable that two of these should be repulsive and one attractive, than as he assumes that two should be attractive and one repulsive :—viz., gross matter and the ether of space attractive, and the elements of the "repulsive envelope" repulsive. In the supposition that the two etheral fluids differ in subtlety,

nothing more is essentially implied than that a considerable number of atoms of the one occupy the interstices between the atoms of the other. Prof. Bayma assumes equally great differences to subsist between his two attractive forms of matter. He remarks, "the distinction of such a medium" (a medium for the transmission of light) "from any ponderable substance, is not an hypothesis but a necessary inference drawn from observed facts;" and again, "I do not see how such a fact" (that light can pass undisturbed through air notwithstanding the immense number of air particles it encounters) "can be accounted for if æther is not immensely denser than atmospheric air." The reason for the conclusion is groundless, but it is the conclusion itself that we have here to notice. He adds "with this great density æther possesses also a very great subtlety."

I might also reply to Prof. Bayma, by asking him why we should admit, in order to explain electric and optical phenomena, two substances so distinct as the repulsive envelop of molecules, and the attractive luminiferous ether. The evidence of their similarity is much greater than of their dissimilarity.

In speaking of the two ethers as subtle, it was meant that a large number of their atoms occupied the interstices between the atoms of gross matter. It was also of course recognized that the velocity of propagation of a wave is much greater through either of the ethereal fluids than through a mass of ordinary matter. The only apparent force in the question under consideration, is derived from the fact that a vague conjecture is apt to be raised by it that a single ether may be equal to all the duty now assigned to both.

To proceed with our quotations:

> What we have said on the constitution of molecules demonstrates indeed the necessity of granting to each molecule of ponderable matter a repulsive atmosphere, which we have called the *molecular envelope.* But this envelope is not of æther, since æther is not repulsive.

That is, is not of the same substance as his luminiferous ether, which he regards as attractive. But the "atmosphere" which corresponds in its direct operation with Prof. Bayma's "molecular envelope" is composed of electric matter, and this is repulsive. It is true that I conceive the interstitial spaces of this electric matter, and the space between it and the central atom to be pervaded by the ether of space; but the mechanical part chiefly played by this condensed universal ether consists in its being the medium in which pulses are originated that constitute the force of *heat repulsion.*

Had Professor Norton known the impossibility of continuous matter, he would have found out that what he calls an atom of gross matter comprises already not only the central element of a molecule, but its nuclei and its envelope; and consequently is already endued with the properties and invested with the arrangements which enable it to exert forces of attraction and repulsion upon other molecules; without requiring any new and special atmosphere of electric or luminiferous æther.

That is, in other words, as already shown, Prof. Bayma's nucleus and envelop are in all outward relations precisely correspondent to my central atom and electric atmosphere. The only essential point of difference between us lies in the fact that I conceive that the interstitial luminiferous ether is condensed around the central atom, and is concerned in the production of some of the phenomena. It is not easy to see how Professor Bayma escapes the conclusion that his interstitial ether, which is attracted by the central nucleus, is condensed around it: still it is plain that he imagines that all natural phenomena are produced by the mutual actions of molecules composed of a central nucleus and a single repulsive envelop, without the intervention of any ether; except the luminiferous, in the case of the phenomena of light and radiant heat. This will appear from the following quotation:

As for the examples, by which he illustrates the theory, they consist of a series of phenomena of different kinds, the explanation of which does not show that the theory is not at fault. For it must be remarked, that those explanations do not imply the existence of extended atoms or of two distinct ætherial substances; and therefore the theory assumes more than is necessary for, or guaranteed by, the explanation of phenomena.

When he has shown this to be true of even the ordinary calorific and electric phenomena, we will admit that his objection to a second ethereal atmosphere interpenetrating the first, may have some force. He has given no hint of the general manner in which he supposes electric phenomena to be evolved. Heat he conceives to originate in the vibrations of the molecules of bodies; but, it can be proved almost to a demonstration, that heat cannot originate in this manner.

Our author proceeds as follows:

The atoms of gross matter being "indivisible" cannot be extended, and cannot be conceived to be "spherical in form, for if they were extended and indivisible they would be so many pieces of continuous matter, which we have already proved to be impossible.

To this I have the following replies to offer.

1. Professor Bayma assumes that every point of matter acts instantaneously upon every other point, at all distances however great or small, with a force having the same character at all distances, and inversely proportional to the square of the distance. This may seem probable, but is not self-evident; and in fact no reason can be assigned why one material point, having no extent, should act upon another with a force decreasing with the distance, according to any law whatever. The law of inverse squares is a consequence of wave propagation, or of radiations along definite lines, received on a molecule of definite size, and cannot be predicated of a force that acts instantaneously between two mathematical points. To suppose such a law is an arbitrary assumption.

2. If matter consists of material points, as supposed by Prof. Bayma, it is no more difficult to conceive of an atom of continuous matter, than of the space coextensive with it.

3. It is not more difficult to conceive of an indivisible atom acting as a whole upon another atom, with a certain energy, than of a mere point acting upon another point, and causing it to change its place; at the same time transferring to a new point all the properties it possesses.

4. If the occult nature of the force of action of one material point on another, be such that the intensity becomes indefinitely small at indefinitely small distances, instead of infinitely great as imagined by Prof. Bayma, then a collection of an infinite number of material points may form one invariable atom. Since the size of the atom may, in every instance, be so inappreciable in comparison with the distance between the nearest atoms, that there may never be any inequality of extraneous action on different points of the same atom, imparting different velocities to them, and so tending to break up the continuity of the matter. Besides, we have already seen that no inequality of elementary action, by reason of a difference of distance is legitimately deducible from Prof. Bayma's premises.

5. In speaking of atoms of gross matter as "indivisible," no other ground was intended to be taken than that each atom was indestructible from any possible action of another atom, and essentially invariable in form. This does not preclude the idea that the atom may be an aggregation of a finite number of material points; for it may be that the mutual action of two attractive points passes into a repulsion at excessively minute distances, and so that an atom of ordinary matter may be a system of material points, in either a statical or dynamical equilibrium. Indivisibility, taken in the only

sense in which the term can properly be used, does not then necessarily imply continuity, as maintained by Prof. Bayma.

6. The assumption that each atom is "spherical in form," was adopted merely as the simplest embodiment of the fundamental principles that the action of the atom was equal in all directions, and that the attractive action upon an atom of ether was neutralized at minute distances by the resistance developed at the point of contact. The existence of such a resistance necessarily implies that the elementary parts of the attractive atom, whether finite or infinite in number, act repulsively at very minute distances. But another conception may be formed of the mode of operation of an atom of gross matter, which involves no other supposition than that it acts equally outward in all directions from a center, and takes no account of its geometrical extent. This is, that *the effective attraction of the atom for the ether of space is due to the existence of a repulsion less than would be exerted by the one or more atoms of ether that would naturally occupy its place.* The result would be the condensation of an atmosphere of ether around the atom, without the exertion of any direct attractive force, or of any additional force of resistance. We may conceive the molecular atmosphere of electric ether to originate in a similar way; but as the opportunity of examining and testing this idea sufficiently has not yet been obtained, I shall continue to regard the electric ether as directly attracted by the atom of gross matter, and that the antagonistic force of resistance is furnished by the repulsion of the luminiferous ether condensed around the atom.

If, in accordance with these views, we seek for a possible origin of *gravitation*, we can find it in a primary attraction subsisting between atoms of gross matter. This must be excessively feeble in comparison with "molecular forces," and modify the effect of those forces only by creating a slight additional pressure of contiguous molecular atmospheres. Should we assume the primary actions between atoms of all kinds, to be wholly repulsive, and the effective attraction of the gross atom for both its etherial atmospheres to be a mere consequence of inequalities of repulsion, it is conceivable that the attraction of gravitation might result from ethereal waves, as maintained by Professor Challis;—these waves having their origin in a *dynamical* equilibrium of the atmosphere of universal ether condensed around each atom.

Another critical remark is the following.

Again, æthereal substance according to the author, is repulsive; now this is inconsistent with astronomical facts, as we have sufficiently shown.

The principal astronomical fact, here referred to, is that the planets do not encounter any sensible resistance in their motion through space. The evidence of an ethereal resistance afforded by Encke's comet, Prof. Bayma strives to explain away without success. The fact that no sensible resistance is experienced by the planets does not necessarily imply, as he supposes, that the ether is not repulsive. For, in the first place, if the molecules of the planetary mass have the constitution I have attributed to them, the impinging ether must take effect upon either the ethereal or the electric atmospheres of the molecules, and so may be mostly expended in the generation of heat and electric currents. I have in fact undertaken to show, in my paper on Molecular Physics, that the earth may derive its magnetic condition, and a certain portion of its heat, from the impact of the ether of space. Again, if the action of gravity be not instantaneous it will take effect in a direction slightly inclined to the radius-vector, and, in the existing state of the planetary system, the tangential component resulting from this inclination may be in equilibrium with the feeble overplus of resistance from the ether. Besides, the supposed difficulty is not removed by substituting an attractive for a repulsive ether. It is true that when a molecule of the earth's mass encounters an atom of the ether on the line of its advance, it will, upon Prof. Bayma's ideas, pass through it, and leave it behind; but he has failed to note the fact that during the approach of the two, their relative velocity will be equal to the sum of the velocity of the earth and that due to their mutual attraction, and during their separation will be equal to the difference of the same velocities, and hence that the atom of ether will continue to attract the molecule during a longer interval of time while the two are separating than while they are approaching. The molecule will therefore on the whole, be retarded by the action of the atom. If the attractive ether be "immensely denser than atmospheric air," the resistance should certainly not be less than that of a subtile repulsive ether. If Prof. Bayma should still hold to the same line of argument, I do not see but he must abolish the ether of space altogether.

He continues:

Moreover, the writer, after having assumed that the electric and luminiferous ethers are both made up of atoms that repel each other assumes also that electric æther attracts luminiferous æther; for he admits that a molecule is formed of an atom of gross matter with two atmospheres, of which the first consisting of condensed luminiferous æther is attracted by the other, which consists of electric æther. Now if the atoms of electric æther are repulsive, how can they attract? So then we must conclude that Prof. Nor-

tons's theory as presented by him, in spite of the talent and learning of its author, cannot be adopted in science.

Prof. Bayma has here entirely misunderstood me, and represented what I threw out as a possible and perhaps probable conception, to be a fundamental principle of my theory. The real fundamental principle was that the atoms of electric ether repelled each other, and it was merely conjectured that this repulsion might be due to atmospheres of luminiferous ether condensed around the electric atoms, instead of being a direct repulsive action. It is a little singular that in view of this distinct statement of the manner in which the repulsion might result from a possible attraction, that our author should ask the question, "Now if the atoms of electric ether are repulsive, how can they attract?", and thereupon intimate the existence of a discrepancy fatal to the theory. It is in fact, altogether immaterial whether the mutual repulsion of electric atoms is indirect as conjectured, or direct.

It has now been made sufficiently apparent that the objections urged against my theory of molecular physics have no real force, and that its fundamental principles have not been disturbed. Whether it will ultimately be "adopted in science" or not, must depend upon its availability in rendering a satisfactory account of phenomena, and its ability to withstand the test of a detailed comparison with the entire range of physical facts. If life and health are granted me, I shall endeavor in good time to show, to the satisfaction of every candid mind, that the natural phenomena and experimental results, with their laws and features of diversity, that make up the different departments of Physics, are legitimately deducible from the fundamental principles of the theory; and that it presents claims to acceptance superior to those which can be urged in favor of any other theory.

The attempt to deduce the existing constitution of things and prominent phenomena, by Prof. Bayma from his fundamental ideas, so far as made, has certainly failed at several important points. To specify one or two of these. He obtains a curve of molecular action, that represents a repulsion at the smallest distances succeeded by an attraction at greater distances. This can only be made to represent the three states of bodies by conceiving the molecules of a gas to be in such a condition that if it were entirely freed from pressure, it would expand into a liquid. We know that many gases can be compressed into a liquid, but it is altogether gratuitous to suppose that they could be brought into a similar condition by a diminution of pressure. Experiment has given no indication of such a result or tendency.

Heat and light he conceives to originate in vibrations of gross molecules, but against this notion, as I shall take another occasion to show, insuperable objections may be urged. If this be given up, his explanation of the changes of the state of bodies must also be abandoned.

The doctrine that "transparent bodies transmit rays of light by the motions of their own molecules" will hardly be accepted, we think, by physicists. It would be a waste of time to argue against it.

Again the notion that a certain substance radiates light of a certain color because its molecules are made to vibrate in unison with the ray of that color, will not stand; for the results of spectral analysis show that the parts of a body which are capable by vibration of giving out any color, are precisely those which absorb and stifle that color. This fact, we may add, also proves conclusively that the rays cannot be transmitted by the motion of the molecules. Though so radically at variance with Prof. Bayma's theoretical views, it is in entire accordance with my own, for according to these, light originates in certain vibratory movements of the atoms of the electric atmospheres of molecules, and when these vibrate naturally in unison with the ray of any color that falls upon them, they take up its vis viva, and so the ray is transformed into a molecular electric current.

As to the "leading principles" laid down by the author, they may in the main be conceded; but these by no means cover the whole ground upon which his theory is raised. We find, for example, that he assumes that all elements or material points of the same form of matter act, under similar circumstances, with the same intensity. Now if this principle be admitted, what theoretical basis have we for the existence of distinct primitive molecules for every different substance;—the number of elements associated together being exactly the same for each primitive molecule of each substance, and different for primitive molecules of different substances. The natural tendency would be to a fortuitous association of elements in an endless variety of numbers, into groups. No controlling principle by which uniformity would be evolved from chaotic confusion, is furnished by the theory. The Hand of the Creator must be supposed to have miraculously interfered, and guided each element to its precise place, in the formation of every molecule of matter. The objection here urged, derives still greater force from the consideration that both the nucleus and envelop of each specific molecule are assumed to have a regular geometrical form, different for each substance. To assume the existence of such molecules, is to make an incalculable number of arbitrary

assumptions. No such exception can be taken to the views I have advocated. For primarily each specific atom of gross attractive matter must appropriate to itself from the universally diffused repulsive ethers, its electric and its ethereal atmosphere, each of a certain definite extent. Upon the relations of these specific atmospheres to the central atom and to one another, all the different proporties of each specific molecule must depend.

We have already seen that the principle that one material point acts upon another, instantaneously, without the intervention of any medium, is opposed to the fundamental idea that the force exerted is inversely proportional to the square of the distance. This law, to say the least, is an arbitrary assumption in the premises. The author also conceives that the mutual action of two material points, is in no degree and under no circumstances intercepted by another intervening point. But we know that in the case of the molecular forces, the amount of vis viva expended in imparting motion to one particle is abstracted from the force in action, and according to Prof. Bayma, the molecular forces are of the same nature as the forces subsisting between the material elements. The force of gravity, it is true, is not sensibly intercepted, but this does not prove that a tendency to interception does not exist; for, upon the supposition of a wave transmission of the force, the effective attraction of any molecule may be the mere differential of the actual force transmitted, and besides, in the circular revolution of a planet the distance from the sun remains unchanged.

My own doctrine is, that the molecular forces, including the heat-repulsion, are dynamical forces, transmitted by wave-propagation and developed by the primary forces of attraction and repulsion, subsisting between the atoms of gross matter and those of the electric matter and the ether of space. The primary forces determine the electric and ethereal atmospheres of molecules, originate the molecular forces proper, and also when an inequality of electric condition is produced on two contiguous molecules or bodies, by molecular actions, gives rise to the special forces of electric attraction and repulsion. The waves of heat and light originate in the ethereal atmospheres of molecules, and are developed by vibrations of the atoms of the electric atmospheres, toward and from the center of each mole cule and the region of ethereal disturbance. By reason of the varying conditions of equilibrium the rate of vibration increases, and its intensity or vis viva, decreases, in proportion as the electric atom is more remote from the center of the molecule. Thus, of the different colored rays, the red proceeds from the lowest depth in the electro-ethereal atmosphere. The obscure heat rays originate at a still lower depth. Heat and light may also

originate in the space between two molecules in the act of combination, or near approach, by reason of the condensation of the interstitial electric ether toward the line of the centers, resulting from the oblique attractive action of the molecules. In this condensation of the electric ether between molecules that are urged nearer to each other, and the expansion of the same when they are separated, we find the key to the explanation of the different modes of electric excitation (that of the galvanic current included). The secret of the intimate relations between electricity and heat and light, is obvious in view of what has been stated.

The ethereal atmospheres of molecules, besides playing the part already signalized, are the chief determining cause of the diverse phenomena that attend the transmission of light through transparent media. Thus *refraction* is chiefly due to the retardation attending the propagation of the ray around from one side to the other of the molecular atmospheres; *dispersion* of the rays in the spectrum, to the fact that the rays of the greatest intensity, and slowest rate of vibration, penetrate to the greatest depth in the molecular atmospheres, pass around in smaller circles, and thus suffer the least retardation; and *double refraction* to the fact that the atmospheres have a spheroidal form, owing to unequal molecular compression on different sides.

Art. XV.—*On Faraday as a Discoverer;* by John Tyndall, F.R.S.*

[Continued from page 51.]

Points of Character.

A point highly illustrative of the character of Faraday now comes into view. He gave an account of his discovery of magneto-electricity in a letter to his friend M. Hachette, of Paris, who communicated the letter to the Academy of Sciences. The letter was translated and published; and immediately afterward two distinguished Italian philosophers took up the subject, made numerous experiments, and published their results before the complete memoirs of Faraday had met the public eye. This evidently irritated him. He reprinted the paper of the learned Italians in the 'Philosophical Magazine,' accompanied by sharp critical notes from himself. He also wrote a letter dated Dec. 1st, 1832, to Gay Lussac, who was then one of the editors of the 'Annales de Chimie,' in which he analyzed the results of the Italian philosophers, pointing out their errors,

* From the Report of the Royal Institution of Great Britain.

and defending himself from what he regarded as imputations on his character. The style of this letter is unexceptionable, for Faraday could not write otherwise than as a gentleman; but the letter shows that had he willed it, he could have hit hard. We have heard much of Faraday's gentleness and sweetness and tenderness. It is all true, but it is very incomplete. You cannot resolve a powerful nature into these elements, and Faraday's character would have been less admirable than it was, had it not embraced forces and tendencies to which the silky adjectives "gentle" and "tender" would by no means apply. Underneath his sweetness and gentleness was the heat of a volcano. He was a man of excitable and fiery nature; but through high self-discipline he had converted the fire into a central glow and motive power of life, instead of permitting it to waste itself in useless passion. "He that is slow to anger," saith the sage, "is greater than the mighty, and he that ruleth his own spirit than he that taketh a city." Faraday was *not* slow to anger, but he completely ruled his own spirit, and thus, though he took no cities, he captivated all hearts.

As already intimated, Faraday had contributed many of his minor papers—including his first analysis of caustic lime—to the 'Quarterly Journal of Science.' In 1832 he collected those papers and others, together in a small octavo volume, labelled them, and prefaced them thus:—

"PAPERS, NOTES, NOTICES, &c., &c.,
published in octavo,
up to 1832.
M. FARADAY."

"*Papers* of mine published in octavo, in *Quarterly Journal of Science*, and elsewhere, since the time that Sir H. Davy encouraged me to write the analysis of caustic lime.

"Some, I think, (at this date) are good; others moderate; and some bad. But I have put *all* into the volume, because of the utility they have been of to me,—and none more than the bad,—in pointing out to me in future, or rather, after times, the faults it became me to watch and to avoid.

"As I never looked over one of my papers a year after it was written, without believing both in philosophy and manner it could have been much better done, I still hope the collection may be of great use to me.

"*Aug.* 18, 1832." "M. FARADAY.

"None more than the bad!" This is a bit of Faraday's innermost nature; and as I read these words, I am almost constrained to retract what I have said regarding the fire and excitability of his character. But is he not all the more admirable

through his ability to tone down and subdue that fire and that excitability, so as to render himself able to write thus as a little child? I once took the liberty of censuring the conclusion of a letter of his to the Dean of St. Paul's. He subscribed himself "humbly yours," and I objected to the adverb. "Well, but, Tyndall," he said, "I *am* humble; and still it would be a great mistake to think that I am not also proud." This duality ran through his character. A democrat in his defiance of all authority which unfairly limited his freedom of thought, and still ready to stoop in reverence to all that was really worthy of reverence, in the customs of the world or the characters of men.

And here, as well as elsewhere, may be introduced a letter which bears upon this question of self-control, written long years subsequent to the period at which we have now arrived. I had been at Glasgow in 1855, at a meeting of the British Association. On a certain day, I communicated a paper to the physical section, which was followed by a brisk discussion. Men of great distinction took part in it, the late Dr. Whewell among the number, and it waxed warm on both sides. I was by no means content with this discussion; and least of all with my own part in it. This discontent affected me for several days, during which I wrote to Faraday, giving him no details, but expressing in a general way my dissatisfaction. I give the following extract from his reply:—

SYDENHAM, *6th Oct.*, 1855.

"MY DEAR TYNDALL,

"These great meetings, of which I think very well altogether, advance science chiefly by bringing scientific men together, and making them to know and be friends with each other; and I am sorry when that is not the effect in every part of their course. I know nothing except from what you tell me, for I have not yet looked at the reports of the proceedings; but let me, as an old man, who ought by this time to have profited by experience, say that when I was younger, I found I often misinterpreted the intentions of people, and found they did not mean what at the time I supposed they meant; and, further, that as a generul rule, it was better to be a little dull of apprehension, where phrases seemed to imply pique, and quick in perception, when on the contrary they seemed to imply kindly feeling. The real truth never fails ultimately to appear; and opposing parties if wrong, are sooner convinced when replied to forbearingly, than when overwhelmed. All I mean to say is, that it is better to be blind to the results of partisanship, and quick to see good will. One has more happiness in oneself, in endeavoring to follow the things that make for peace. You can hardly imagine how often I have been heated in private when opposed, as I have thought unjustly and superciliously, and yet I have striven, and succeeded I hope, in keeping down replies

of the like kind. And I know I have never lost by it. I would not say all this to you did I not esteem you as a true philosopher and friend.*

"Yours, very truly, M. FARADAY."

Identity of Electricities: First Researches on Electro-Chemistry.

I have already once used the word "discomfort" in reference to the occasional state of Faraday's mind when experimenting. It was to him a discomfort to reason upon data which admitted of doubt. He hated what he called "doubtful knowledge," and ever tended either to transfer it into the region of undoubtful knowledge, or of certain and definite ignorance. Pretence of all kinds, whether in life or philosophy, was hateful to him. He wished to know the reality of our nescience as well as of our science. "Be one thing or the other," he seemed to say to an unproved hypothesis, "come out as a solid truth, or disappear as a convicted lie." After making the great discovery which I have attempted to describe, a doubt seemed to beset him as regards the identity of electricities. "Is it right," he seemed to ask, "to call this agency which I have discovered, electricity at all? Are there perfectly conclusive grounds for believing that the electricity of the machine, the pile, the gymnotus and torpedo, magneto-electricity and thermo-electricity, are merely different manifestations of one and the same agent?" To answer this question to his own satisfaction, he formally reviewed the knowledge of that day. He added to it new experiments of his own, and finally decided in favor of the "Identity of Electricities." His paper upon this subject was read before the Royal Society on the 10th and 17th of January, 1833.

After he had proved to his own satisfaction the identity of electricities, he tried to compare them quantitatively together. The terms quantity and intensity, which Faraday constantly used, need a word of explanation here. He might charge a single Leyden jar by twenty turns of his machine, or he might charge a battery of ten jars by the same number of turns. The *quantity* in both cases would be sensibly the same, but the *intensity* of the single jar would be the greatest, for here the electricity would be less diffused. Faraday first satisfied himself that the needle of his galvanometer was caused to swing through the same arc by the same quantity of machine electricity, whether it was condensed in a small battery or diffused over a

* Faraday would have been rejoiced to learn that, during its last meeting at Dundee, the British Association illustrated in a striking manner the function which he here describes as its principal one. In my own case, a brotherly welcome was everywhere manifested. In fact, the differences of really honorable and sane men are never beyond healing.

large one. Thus the electricity developed by thirty turns of his machine produced, under very variable conditions of battery surface, the same deflection. Hence he inferred the possibility of comparing, as regards quantity, electricities which differ greatly from each other in intensity.

His object now is, to compare frictional with voltaic electricity. Moistening bibulous paper with the iodid of potassium —a favorite test of his—and subjecting it to the action of machine electricity, he decomposed the iodid, and formed a brown spot where the iodid was liberated. Then he immersed two wires, one of zinc, the other of platinum, each $\frac{1}{18}$th of an inch in diameter, to a depth of $\frac{5}{8}$ths of an inch in acidulated water during eight beats of his watch, or $\frac{3}{20}$ths of a second; and found that the needle of his galvanometer swung through the same arc, and colored his moistened paper to the same extent, as thirty turns of his large electrical machine. Twenty-eight turns of the machine produced an effect distinctly less than that produced by his two wires. Now the quantity of water decomposed by the wires in this experiment, totally eluded observation; it was immeasurably small; and still that amount of decomposition involved the development of a quantity of electric force which, if applied in its proper form would kill a rat, and no man would like to bear it.

In his subsequent researches "on the absolute quantity of electricity associated with the particles or atoms of matter," he endeavors to give an idea of the amount of electrical force involved in the decomposition of a single grain of water. He is almost afraid to mention it, for he estimates it at 800,000 discharges of his large Leyden battery. This, if concentrated in a single discharge, would be equal to a very great flash of lightning; while the chemical action of a single grain of water on four grains of zinc would yield electricity equal in quantity to a powerful thunderstorm. Thus his mind rises from the minute to the vast, expanding involuntarily from the smallest laboratory fact till it embraces the largest and grandest of natural phenomena.*

In reality, however, he is at this time only clearing his way, and he continues laboriously to clear it for some time afterward. He is digging the shaft, guided by that instinct toward the

* Buff finds the quantity of electricity associated with one milligram of hydrogen in water, to be equal to 45.480 charges of a Leyden jar, with a height of 480 millimetres, and a diameter of 160 millimetres. Weber and Kohlrausch have calculated that if the quantity of electricity associated with one milligram of hydrogen in water, were diffused over a cloud at a height of 1,000 metres above the earth, it would exert upon an equal quantity of the opposite electricity at the earth's surface an attractive force of 2,268,000 kilograms.—*Electrolytische Maasbestimmungen*, 1866, p. 262.

mineral lode which was to him a rod of divination. "*Er riecht die Wahrheit,*" said the lamented Kohlrausch, an eminent German, once in my hearing:—"He smells the truth." His eyes are now steadily fixed on this wonderful voltaic current, and he must learn more of its mode of transmission.

On the 23d of May, 1833, he read a paper before the Royal Society, "On a new Law of Electric Conduction." He found that though the current passed through water, it did not pass through ice:—why not, since they are one and the same substance? Some years subsequently he answered this question by saying that the liquid condition enables the molecule of water to turn round so as to place itself in the proper line of polarization, while the rigidity of the solid condition prevents this arrangement. This polar arrangement must prècede decomposition, and decomposition is an accompaniment of conduction. He then passed on to other substances; to oxdys and chlorids, and iodids, and salts, and sulphurets, and found them all insulators when solid, and conductors when fused. In all cases moreover, except one—and this exception he thought might be apparent only—he found the passage of the current across the fused compound to be accompanied by its decomposition. Is then the act of decomposition essential to the act of conduction in these bodies? Even recently this question was warmly contested. Faraday was very cautious latterly in expressing himself upon this subject; but as a matter of fact he held that an infinitesimal quantity of electricity might pass through a compound liquid without producing its decomposition. De la Rive, who has been a great worker on the chemical phenomena of the pile, is very emphatic on the other side. Experiment, according to him and others, establishes in the most conclusive manner that no trace of electricity can pass through a liquid compound without producing its equivalent decomposition.*

Faraday has now got fairly entangled amid the chemical phenomena of the pile, and here his previous training under Davy must have been of the most important service to him. Why, he asks, should decomposition thus take place? what force is it that wrenches the locked constituent of these compounds asunder? On the 20th of June, 1833, he read a paper before the Royal Society "On Electro-Chemical Decomposition," in which he seeks to answer these questions. The notion has been entertained that the poles as they are called, of the decomposing cell, or in other words the surfaces by which the current enters and quits the liquid, exercised electric attractions upon

* 'Faraday, sa Vie et ses Travaux,' p. 20.

the constituents of the liquid and tore them asunder. Faraday combats this notion with extreme vigor. Litmus reveals, as you know, the action of an acid by turning red, turmeric reveals the action of an alkali by turning brown. Sulphate of soda, you know, is a salt compounded of the alkali soda and sulphuric acid. The voltaic current passing through a solution of this salt so decomposes it, that sulphuric acid appears at one pole of the decomposing cell and alkali at the other. Faraday steeped a piece of litmus paper and a piece of turmeric paper in a solution of sulphate of soda; placing each of them upon a separate plate of glass, he connected them together by means of a string moistened with the same solution. He then attached one of them to the positive conductor of an electric machine, and the other to the gas-pipes of this building. These he called his "discharging train." On turning the machine, the electricity passed from paper to paper through the string, which might be varied in length from a few inches to seventy feet without changing the result. The first paper was reddened, declaring the presence of sulphuric acid; the second was browned, declaring the presence of the alkali soda. The dissolved salt, therefore, arranged in this fashion was decomposed by the machine exactly as it would have been by the voltaic current. When instead of using the positive conductor he used the negative, the positions of the acid and alkali were reversed. Thus he satisfied himself that chemical decomposition by the machine is obedient to the laws which rule decomposition by the pile.

And now he gradually abolishes these so-called poles to the attraction of which electric decomposition had been ascribed. He connected a piece of turmeric paper moistened with the sulphate of soda with the positive conductor of his machine; then he placed a metallic point in connection with his discharging train opposite the moist paper, so that the electricity shall discharge through the air toward the point. The turning of the machine caused the corners of the piece of turmeric paper opposite to the point to turn brown, thus declaring the presence of alkali. He changed the turmeric for litmus paper, and placed it not in connection with his conductor, but with his discharging train, a metallic point connected with the conductor being fixed at a couple of inches from the paper; on turning the machine, acid was liberated at the edges and corners of the litmus. He then placed a series of pointed pieces of paper, each separate piece being composed of two halves, one of litmus and the other of turmeric paper, and all moistened with sulphate of soda, in the line of current from the machine. The pieces of paper were separated from each other by spaces of air. The machine was turned; and it was always

found that at the point where the electricity entered the paper, litmus was reddened, and at the point where it quitted the paper, turmeric was browned. "Here," he urges, "the poles are entirely abandoned, but we have still electro-chemical decomposition." It is evident to him that instead of being *attracted* by the poles, the bodies separated are *ejected* by the current. The effects thus obtained with poles of air he also succeeded in obtaining with poles of water. The advance in Faraday's own ideas made at this time is indicated by the word "ejected." He afterwards reiterates this view: the evolved substances are *expelled* from the decomposing body and "*not drawn out by an attraction.*"

Having abolished this idea of polar attraction, he proceeds to enunciate and develop a theory of his own. He refers to Davy's celebrated Bakerian Lecture given in 1806, which he says "is almost entirely occupied in the consideration of electro-chemical decompositions." The facts recorded in that lecture Faraday regards as of the utmost value. But "the mode of action by which the effects take place is stated very generally; so generally indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing essentially from each other, yet all agreeing with the statement there given."

It appears to me that these words might with justice be applied to Faraday's own researches at this time. They furnish us with results of permanent value; but little help can be found in the theory advanced to account for them. It would, perhaps, be more correct to say that the theory itself is hardly presentable in any tangible form to the intellect. Faraday looks, and rightly looks, into the heart of the decomposing body itself: he sees, and rightly sees, active within it the forces which produce the decomposition, and he rejects, and rightly rejects, the notion of external attraction; but beyond the hypothesis of decompositions and recompositions, enunciated and developed by Grothuss and Davy, he does not, I think, help us to any definite conception as to how the force reaches the decomposing mass and acts within it. Nor, indeed, can this be done, until we know the true physical process which underlies what we call an electric current.

Faraday conceives of that current as "*an axis of power having contrary forces exactly equal in amount in opposite directions:*" but this definition, though much quoted and circulated, teaches us nothing regarding the current. An "axis" here can only mean a direction; and what we want to be able to conceive of is, not the axis along which the power acts, but the nature and mode of action of the power itself. He objects

to the vagueness of De la Rive; but the fact is that both he and De la Rive labor under the same difficulty. Neither wishes to commit himself to the notion of a current compounded of two electricities flowing in two opposite directions; but the time had not come, nor is it yet come, for the displacement of this provisional fiction by the true mechanical conception. Still, however indistinct the theoretic notions of Faraday at this time may be, the facts which are arising before him and around him are leading him gradually, but surely, to results of incalculable importance in relation to the philosophy of the voltaic pile.

He had always some great object of research in view, but in the pursuit of it, he frequently alighted on facts of collateral interest, to examine which he sometimes turned aside from his direct course. Thus we find the series of his researches on electro-chemical decomposition interrupted by an inquiry into "the power of metals and other solids, to induce the combination of gaseous bodies." This inquiry, which was received by the Royal Society on the 30th of November, 1833, though not so important as those which precede and follow it, illustrates throughout his strength as an experimenter. The power of spongy platinum to cause the combination of oxygen and hydrogen had been discovered by Döbereiner in 1823, and had been applied by him in the construction of his well-known philosophic lamp. It was shown subsequently by Dulong and Thenard that even a platinum wire, when perfectly cleansed, may be raised to incandescence by its action on a jet of cold hydrogen.

In his experiments on the decomposition of water, Faraday found that the positive platinum plate of the decomposing cell possessed in an extraordinary degree, the power of causing oxygen and hydrogen to combine. He traced the cause of this to the perfect cleanness of the positive plate. Against it was liberated oxygen, which with the powerful affinity of the "nascent state," swept away all impurity from the surface against which it was liberated. The bubbles of gas liberated on one of the platinum plates or wires of a decomposing cell are always much smaller, and they rise in much more rapid succession than those from the other. Knowing that oxygen is sixteen times heavier than hydrogen, I have more than once concluded, and, I fear, led others into the error of concluding, that the smaller and more quickly rising bubbles must belong to the lighter gas. The thing appeared so obvious that I did not give myself the trouble of looking at the battery, which would at once have told me the nature of the gas. But Faraday would never have

been satisfied with a deduction if he could have reduced it to a fact. And he has taught me that the fact here is the direct reverse of what I supposed it to be. The small bubbles are oxygen, and their smallness is due to the perfect cleanness of the surface on which they are liberated. The hydrogen adhering to the other electrode swells into large bubbles, which rise in much slower succession; but when the current is reversed, the hydrogen is liberated upon the cleansed wire, and then its bubbles also become small.

Laws of Electro-Chemical Decomposition.

In our conceptions and reasonings regarding the forces of nature, we perpetually make use of symbols which, when they possess a high representative value, we dignify with the name of theories. Thus prompted by certain analogies, we ascribe electrical phenomena to the action of a peculiar fluid, sometimes flowing, sometimes at rest. Such conceptions have their advantages and their disadvantages; they afford peaceful lodging to the intellect for a time, but they also circumscribe it, and by and by, when the mind has grown too large for its lodging, it often finds difficulty in breaking down the walls of what has become its prison instead of its home.*

No man ever felt the tyranny of symbols more deeply than Faraday, and no man was ever more assiduous than he to liberate himself from them and the terms which suggested them. Calling Dr. Whewell to his aid in 1833, he endeavored to displace by others all terms tainted by a foregone conclusion. His paper on Electro-chemical decomposition, received by the Royal Society on the 9th of January, 1834, opens with the proposal of a new terminology. He would avoid the word "current" if he could.† He does abandon the word "poles" as applied to the ends of a decomposing cell, because it suggests the idea of attraction, substituting for it the perfectly neutral term *electrodes.* He applied the term *electrolyte* to every substance which can be decomposed by the current, and the act of decomposition, he calls *electrolysis.* All these terms have become current in science. He called the positive electrode the *Anode*, and the negative one the *Cathode*, but these terms, though frequently used, have not enjoyed the same currency as

* I copy these words from the printed abstract of a Friday evening lecture, given by myself, because they remind me of Faraday's voice responding to the utterance by an emphatic *hear! hear!—Proceedings of the Royal Institution*, vol. ii, p. 132.

† In 1838 he expresses himself thus:—"The word current is so expressive in common language that when applied in the consideration of electrical phenomena, we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it."—*Exp. Researches*, vol. i, p. 515, (§ 1617.)

the others. The terms *Anion* and *Cation*, which he applied to the constituents of the decomposed electrolyte, and the term *ion*, which included both anions and cations, are still less frequently employed.

Faraday now passes from terminology to research; he sees the necessity of quantitive determinations, and seeks to supply himself with a measure of voltaic electricity. This he finds in the quantity of water decomposed by the current. He tests this measure in all possible ways, to assure himself that no error can arise from its employment. He places in the course of one and the same current, a series of cells with electrodes of different sizes, some of them plates of platinum, others merely platinum wires, and collects the gas liberated on each distinct pair of electrodes. He finds the quantity of gas to be the same for all. Thus he concludes that when the same quantity of electricity is caused to pass through a series of cells containing acidulated water, the electro-chemical action is independent of the size of the electrodes. He next proves that variations in intensity do not interfere with this equality of action. Whether his battery is charged with strong acid or with weak; whether it consists of five pairs or fifty pairs; in short, whatever be its source, when the same current is sent through his series of cells, the same amount of decomposition takes place in all. He next assures himself that the strength or weakness of his dilute acid does not interfere with this law. Sending the same current through a series of cells containing mixtures of sulphuric acid and water of different strengths, he finds, however the proportion of acid to water might vary, the same amount of gas to be collected in all the cells. A crowd of facts of this character forced upon Faraday's mind the conclusion that the amount of electro-chemical decomposition depends, not upon the size of the electrodes, not upon the intensity of the current, not upon the strength of the solution, but solely upon the quantity of electricity which passes through the cell. The quantity of electricity, he concludes, is proportional to the amount of chemical action. On this law Faraday based the construction of his celebrated voltameter, or measurer of voltaic electricity.

But before he can apply this measure he must clear his ground of numerous possible sources of error. The decomposition of his acidulated water is certainly a *direct* result of the current; but as the varied and important researches of MM. Becquerel, De la Rive, and others had shown, there are also *secondary* actions, which may materially interfere with and complicate the pure action of the current. These actions may occur in two ways: either the liberated *ion* may seize upon the electrode against which it is set free, forming a chemical compound with

that electrode; or it may seize upon the substance of the electrolyte itself, and thus introduce into the circuit chemical actions over and above those due to the current. Faraday subjected these secondary actions to an exhaustive examination. Instructed by his experiments, and rendered competent by them to distinguish between primary and secondary results, he proceeds to establish the doctrine of "definite electro-chemical decomposition."

Into the same circuit he introduced his voltameter, which consisted of a graduated tube filled with acidulated water and provided with platinum plates for the decomposition of the water, and also a cell containing chlorid of tin. Experiments already referred to had taught him that this substance, though an insulator when solid, is a conductor when fused, the passage of the current being always accompanied by the decomposition of the chlorid. He wished now to ascertain what relation this decomposition bore to that of the water in his voltameter.

Completing his circuit, he permitted the current to continue until "a reasonable quantity of gas" was collected in the voltameter. The circuit was then broken, and the quantity of tin liberated, compared with the quantity of gas. The weight of the former was 3·2 grains, that of the latter 0·49742 of a grain. Oxygen, as you know, unites with hydrogen in the proportion of 8 to 1 to form water. Calling the equivalent, or, as it is sometimes called, the atomic weight of hydrogen 1, that of oxygen is 8; that of water is consequently $8+1$, or 9. Now if the quantity of water decomposed in Faraday's experiment be represented by the number 9, or in other words, by the equivalent of water, then the quantity of tin liberated from the fused chlorid is found by an easy calculation to be 57·9, which is almost exactly the chemical equivalent of tin. Thus both the water and the chlorid were broken up in proportions expressed by their respective equivalents. The amount of electric force which wrenched asunder the constituents of the molecule of water was competent, and neither more nor less than competent, to wrench asunder the constituents of the molecules of the chlorid of tin. This fact is typical. With the indications of his voltameter he compared the decomposition of other substances both singly and in series. He submitted his conclusions to numberless tests. He purposely introduced secondary actions. He endeavored to hamper the fulfilment of those laws which it was the intense desire of his mind to see established. But from all these difficulties, emerged the golden truth, that under every variety of circumstances, the decompositions of the voltaic current are as definite in their character as those chemical combinations which gave birth to the atomic theory. This law of

electro-chemical decomposition ranks, in point of importance, with that of definite combining proportions in chemistry.

Origin of Power in the Voltaic Pile.

In one of the public areas of the town of Como stands a statute, with no inscription on its pedestal save that of a single name, "Volta." The bearer of that name occupies a place for ever memorable in the history of science. To him we owe the discovery of the voltaic pile, to which, for a brief interval, we must now turn our attention.

The objects of scientific thought being the passionless laws and phenomena of external nature, one might suppose that their investigation and discussion would be completely withdrawn from the region of the feelings, and pursued by the cold dry light of the intellect alone. This, however, is not always the case. Man carries his heart with him into all his works. You cannot separate the moral and emotional from the intellectual; and thus it is that the discussion of a point of science may rise to the heat of a battle-field. The fight between the rival optical theories of Emission and Undulation was of this fierce character; and scarcely less fierce for many years was the contest as to the origin and maintenance of the power of the voltaic pile. Volta himself supposed it to reside in the contact of different metals. Here was exerted his "electro-motive force," which tore the combined electricities asunder and drove them as currents in opposite directions. To render the circulation of the current possible, it was necessary to connect the metals by a moist conductor; for when any two metals were connected by a third, their relation to each other was such that a complete neutralization of the electric motion was the result. Volta's theory of metallic contact was so clear, so beautiful, and apparently so complete, that the best intellects of Europe accepted it as the expression of natural law.

Volta himself knew nothing of the chemical phenomena of the pile; but as soon as these became known, suggestions and intimations appeared that chemical action, and not metallic contact, might be the real source of voltaic electricity. This idea was expressed by Fabroni in Italy and by Wollaston in England. It was developed and maintained by those "admirable electricians," Becquerel, of Paris, and De la Rive, of Geneva. The contact theory, on the other hand, received its chief development and illustration in Germany. It was long the scientific creed of the great chemists and natural philosophers of that country, and to the present hour there may be some of them unable to liberate themselves from the fascination of their first-love.

After the researches which I have endeavored to place before you, it was impossible for Faraday to avoid taking a side in this controversy. He did so in a paper "On the Electricity of the Voltaic Pile," received by the Royal Society, on the 7th of April, 1834. His position in the controversy might have been predicted. He saw chemical effects going hand-in-hand with electrical effects, the one being proportional to the other; and, in the paper now before us, he proved that when the former were excluded, the latter were sought for in vain. He produced a current without metallic contact; he discovered liquids which, though competent to transmit the feeblest currents—competent therefore to allow the electricity of contact to flow through them if it were able to form a current—were absolutely powerless when chemically inactive.

One of the very few experimental mistakes of Faraday occurred in this investigation. He thought that with a single voltaic cell he had obtained the spark *before the metals touched*, but he subsequently discovered his error. To enable the voltaic spark to pass through air before the terminals of the battery were united, it was necessary to exalt the electro-motive force of the battery by multiplying its elements; but all the elements Faraday possessed were unequal to the task of urging the spark across the shortest measurable space of air. Nor, indeed, could the action of the battery, the different metals of which were in contact with each other, decide the point in question. Still as regards the identity of electricities from various sources, it was at that day of great importance to determine whether or not the voltaic current could jump as a spark across an interval before contact. Faraday's friend, Mr. Gassiot, solved this problem. He erected a battery of 4000 cells, and with it urged a stream of sparks from terminal to terminal, when separated from each other by a measurable space of air.

The memoir on the "Electricity of the Voltaic Pile," published in 1834, appears to have produced but little impression upon the supporters of the contact theory. These indeed were men of too great intellectual weight and insight lightly to take up, or lightly to abandon a theory. Faraday therefore resumed the attack in a paper communicated to the Royal Society, on the 6th of February, 1840. In this paper he hampered his antagonists by a crowd of adverse experiments. He hung difficulty after difficulty about the neck of the contact theory, until in its efforts to escape from his assaults it so changed its character as to become a thing totally different from the theory proposed by Volta. The more persistently it was defended, however, the more clearly did it show itself to be a congeries of devices, bearing the stamp of dialectic skill rather than that of natural truth.

In conclusion, Faraday brought to bear upon it an argument which, had its full weight and purport been understood at the time, would have instantly decided the controversy. "The contact theory," he urged, "assumes that a force which is able to overcome powerful resistance, as for instance that of the conductors, good or bad, through which the current passes, and that again of the electrolytic action where bodies are decomposed by it, *can arise out of nothing:* that without any change in the acting matter, or the consumption of any generating force, a current shall be produced which shall go on forever against a constant resistance, or only be stopped, as in the voltaic trough, by the ruins which its exertion has heaped up in its own course. This would indeed be *a creation of power*, and is like no other force in nature. We have many processes by which the *form* of the power may be so changed, that an apparent *conversion* of one into the other takes place. So we can change chemical force into the electric current, or the current into chemical force. The beautiful experiments of Seebeck and Peltier show the convertibility of heat and electricity; and others by Oersted and myself show the convertibility of electricity and magnetism. *But in no case, not even in those of the Gymnotus and Torpedo, is there a pure creation or a production of power without a corresponding exhaustion of something to supply it.*"

These words were published more than two years before either Mayer printed his brief but celebrated essay on the Forces of Inorganic Nature, or Mr. Joule published his first famous experiments on the Mechanical Value of Heat. They illustrate the fact that before any great scientific principle receives distinct enunciation by individuals, it dwells more or less clearly in the general scientific mind. The intellectual plateau is already high, and our discoverers are those who, like peaks above the plateau, rise a little above the general level of thought at the time.

But many years prior, even to the foregoing utterance of Faraday, a similar argument had been employed. I quote here with equal pleasure and admiration the following passage written by Dr. Roget so far back as 1829. Speaking of the contact theory, he says:—"If there could exist a power having the property ascribed to it by the hypothesis, namely, that of giving continual impulse to a fluid in one constant direction, without being exhausted by its own action, it would differ essentially from all the known powers in nature. All the powers and sources of motion with the operation of which we are acquainted, when producing these peculiar effects, *are expended in the same proportion as those effects are produced; and hence*

arises the impossibility of obtaining by their agency a perpetual effect; or in other words a perpetual motion. But the electro-motive force, ascribed by Volta to the metals when in contact is a force which, as long as a free course is allowed to the electricity it sets in motion, is never expended, and continues to be excited with undiminished power in the production of a never-ceasing effect. Against the truth of such a supposition the probabilities are all but infinite." When this argument, which he employed independently, had clearly fixed itself in his mind, Faraday never cared to experiment further on the source of electricity in the voltaic pile. The argument appeared to him "to remove *the foundation itself* of the contact theory," and he afterwards let it crumble down in peace.*

Researches on Frictional Electricity: Induction: Conduction: Specific Inductive Capacity: Theory of Contiguous Particles.

The burst of power which had filled the four preceding years with an amount of experimental work unparalleled in the history of science partially subsided in 1835, and the only scientific paper contributed by Faraday in that year was a comparatively unimportant one, "On an improved Form of the Voltaic Battery." He brooded for a time: his experiments on electrolysis had long filled his mind; he looked, as already stated, into the very heart of the electrolyte, endeavoring to render the play of its atoms visible to his mental eye. He had no doubt that in this case what is called "the electric current" was propagated from particle to particle of the electrolyte; he accepted the doctrine of decomposition and recomposition which, according to Grothuss and Davy, ran from electrode to electrode. And the thought impressed him more and more that ordinary electric induction was also transmitted and sustained by the action of "*contiguous particles.*"

His first great paper on frictional electricity was sent to the Royal Society on the 30th of November, 1837. We here find him face to face with an idea which beset his mind throughout his whole subsequent life,—the idea of *action at a distance.* It

* To account for the *electric current*, which was really the core of the whole discussion, Faraday demonstrated the impotence of the contact theory as then enunciated and defended. Still, it is certain that two different metals, when brought into contact, charge themselves, the one with positive and the other with negative electricity. I had the pleasure of going over this ground with Kohlrausch in 1849, and his experiments left no doubt upon my mind that the contact electricity of Volta was a reality, though it could produce no current. With one of the beautiful instruments devised by himself, Sir William Thompson has rendered this point capable of sure and easy demonstration; and he and others now hold what may be called *a* contact theory, which, while it takes into account the action of the metals, also embraces the chemical phenomena of the circuit. Helmholtz, I believe, was the first to give the contact theory this new form, in his celebrated essay, *Ueber die Erhaltung der Kraft*, p. 45.

perplexed and bewildered him. In his attempts to get rid of this perplexity he was often unconsciously rebelling against the limitations of the intellect itself. He loved to quote Newton upon this point: over and over again he introduces his memorable words, "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through *vacuum* and without the mediation of anything else, by and through which this action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers."*

Faraday does not see the same difficulty in his contiguous particles. And yet by transferring the conception from masses to particles we simply lessen size and distance, but we do not alter the quality of the conception. Whatever difficulty the mind experiences in conceiving of action at sensible distances, besets it also when it attempts to conceive of action at insensible distances. Still the investigation of the point whether electric and magnetic effects were wrought out through the intervention of contiguous particles or not, had a physical interest altogether apart from the metaphysical difficulty. Faraday grapples with the subject experimentally. By simple intuition he sees that action at a distance must be exerted in straight lines. Gravity, he knows, will not turn a corner, but exerts its pull along a right line; hence his aim and effort to ascertain whether electric action ever takes place in curved lines. This once proved, it would follow that the action is carried on *by means of a medium* surrounding the electrified bodies. His experiments in 1837, reduced, in his opinion, this point to demonstration. He then found that he could electrify by induction an insulated sphere placed completely in the shadow of a body which screened it from direct action. He pictured the lines of electric force bending round the edges of the screen, and reuniting on the other side of it; and he proved that in many cases the augmentation of the distance between his insulated sphere and the inducing body, instead of lessening, increased the charge of the sphere. This he ascribed to the coalescence of the lines of electric force at some distance behind the screen.

Faraday's theoretic views on this subject have not received general acceptance, but they drove him to experiment, and experiment with him was always prolific of results. By suitable

* Newton's third letter to Bentley.

arrangements he places a metallic sphere in the middle of a large hollow sphere, leaving a space of something more than half an inch between them. The interior sphere was insulated, the external one uninsulated. To the former he communicated a definite charge of electricity. It acted by induction upon the concave surface of the latter, and he examined how this act of induction was affected by placing insulators of various kinds between the two spheres. He tried gases, liquids, and solids, but the solids alone gave him positive results. He constructed two instruments of the foregoing description, equal in size and similar in form. The interior sphere of each communicated with the external air by a brass stem ending in a knob. The apparatus was virtually a Leyden jar, the two coatings of which were the two spheres, with a thick and variable insulator between them. The amount of charge in each jar was determined by bringing a proof-plane into contact with its knob, and measuring by a torsion balance the charge taken away. He first charged one of his instruments, and then dividing the charge with the other, found that when air intervened in both cases, the charge was equally divided. But when shell-lac, sulphur, or spermaceti was interposed between the two spheres of one jar, while air occupies this interval in the other, then he found that the instrument occupied by the "solid dielectric" took *more than half* the original charge. A portion of the charge was absorbed in the dielectric itself. The electricity took time to penetrate the dielectric. Immediately after the discharge of the apparatus no trace of electricity was found upon its knob. But after a time electricity was found there, the charge having gradually returned from the dielectric in which it had been lodged. Different insulators posses this power of permitting the charge to enter them in different degrees. Faraday figured their particles as polarized, and he concluded that the force of induction is propagated from particle to particle of the dielectric from the inner sphere to the outer one. This power of propagation possessed by insulaters he calls their "*Specific Inductive Capacity.*"

Faraday visualizes with the utmost clearness the state of his contiguous particles; one after another they become charged, each succeeding particle depending for its charge upon its predecessor. And now he seeks to break down the wall of partition between conductors and insulators. "Can we not" he says, "by a gradual chain of association carry up discharge from its occurrence in air through spermaceti and water to solutions, and then on to chlorids, oxyds, and metals, without any essential change in its character?" Even copper, he urges, offers a resistance to the transmission of electricity. The action

of its particles differs from those of an insulator only in degree. They are charged like the particles of the insulator, but they discharge with greater ease and rapidity; and this rapidity of molecular discharge is what we call conduction. Conduction then is always preceded by atomic induction; and when through some quality of the body, which Faraday does not define, the atomic discharge is rendered slow and difficult, conduction passes into insulation.

Though they are often obscure, a fine vein of philosophic thought runs through those investigations. The mind of the philosopher dwells amid those agencies which underlie the visible phenomena of Induction and Conduction; and he tries by the strong light of his imagination to see the very molecules of his dielectrics. It would, however, be easy to criticize these researches, easy to show the looseness, and sometimes the inaccuracy, of the phraseology employed; but this critical spirit will get little good out of Faraday. Rather let those who ponder his works seek to realize the object he set before him, not permitting his occasional vagueness to interfere with their appreciation of his speculations. We may see the ripples, and eddies, and vortices of a flowing stream, without being able to resolve all these motions into their constituent elements; and so it sometimes strikes me that Faraday clearly saw the play of fluids and ethers and atoms, though his previous training did not enable him to resolve what he saw into its constituents, or describe it in a manner satisfactory to a mind versed in mechanics. And then again occur, I confess, dark sayings, difficult to be understood, which disturb my confidence in this conclusion. It must, however, always be remembered that he works at the very boundaries of our knowledge, and that his mind habitually dwells in the "boundless contiguity of shade" by which that knowledge is surrounded.

In the researches now under review the ratio of speculation and reasoning to experiment is far higher than in any of Faraday's previous works. Amid much that is entangled and dark we have flashes of wondrous insight and utterances which seem less the product of reasoning than of revelation. I will confine myself here to one example of this divining power:—By his most ingenious device of a rapidly rotating mirror, Wheatstone had proved that electricity required time to pass through a wire, the current reaching the middle of the wire later than its two ends. "If," says Faraday, "the two ends of the wire in Professor Wheatstone's experiments were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the inter-

nal portion of the wire at the first instance, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before. And if those two plates were the inner and outer coatings of a large jar or Leyden battery, then the retardation of the spark would be much greater." This was only a *prediction*, for the experiment was not made.* Sixteen years subsequently, however, the proper conditions came into play, and Faraday was able to show that the observations of Werner Siemens, and Latimer Clark, on subterraneous and submarine wires were illustrations, on a grand scale, of the principle which he had enunciated in 1838. The wires and the surrounding water act as a Leyden jar, and the retardation of the current predicted by Faraday manifests itself in every message sent by such cables.

The meaning of Faraday in these memoirs on Induction and Conduction is, as I have said, by no means always clear; and the difficulty will be most felt by those who are best trained in ordinary theoretic conceptions. He does not know the reader's needs, and he therefore does not meet them. For instance, he speaks over and over again of the impossibility of charging a body with one electricity, though the impossibility is by no means evident. The key to the difficulty is this. He looks upon every insulated conductor as the inner coating of a Leyden jar. An insulated sphere in the middle of a room is to his mind such a coating; the walls are the outer coating, while the air between both is the insulator, across which the charge acts by induction. Without this reaction of the walls upon the sphere you could no more, according to Faraday, charge it with electricity, than you could charge a Leyden jar, if its outer coating were removed. Distance with him is immaterial. His strength as a generalizer enables him to dissolve the idea of magnitude; and if you abolished the walls of the room—even the earth itself—he would make the sun and planets the outer coating of his jar. I dare not contend that Faraday in these memoirs made all his theoretic positions good. But a pure vein of philosophy runs through these writings; while his experiments and reasonings on the forms and phenomena of electrical discharge are of imperishable importance.

Rest needed—Visit to Switzerland.

The last of these memoirs was dated from the Royal Institution in June, 1838. It concludes the first volume of his

* If Sir Charles Wheatstone could be induced to take up his measurements once more, varying the substances througn which, and the conditions under which the current is propagated, he might render great service to science, both theoretic and experimental.

"Experimental Researches on Electricity." In 1840, as already stated, he made his final assault on the contact theory, from which it never recovered.* He was now feeling the effects of the mental strain to which he had been subjected for so many years. During these years he repeatedly broke down. His wife alone witnessed the extent of his prostration, and to her loving care we, and the world, are indebted for the enjoyment of his presence here so long. He found occasional relief in a theatre. He frequently quitted London and went to Brighton and elsewhere, always choosing a situation which commanded a view of the sea, or of some other pleasant horizon, where he could sit and gaze, and feel the gradual revival of the faith that

> "Nature never did betray
> The heart that loved her!"

But very often, for some days after his removal into the country, he would be unable to do more than sit at a window and look out upon the sea and sky.

In 1841, his state became more serious than it had ever been before. A published letter to Mr. Richard Taylor, dated March 11, 1843, contains an allusion to his previous condition. "You are aware," he says, "that considerations regarding health have prevented me from working or reading on science for the last two years." This, at one period or another of their lives, seems to be the fate of most great investigators. They do not know the limits of their constitutional strength until they have transgressed them. It is, perhaps, right that they should transgress them, in order to ascertain where they lie. Faraday, however, though he went far toward it, did not push his transgression beyond his power of restitution. In 1841, Mrs. Faraday and he went to Switzerland, under the affectionate charge of her brother, Mr. George Barnard, the artist. This time of suffering throws fresh light upon his character. I have said that sweetness and gentleness were not its only constituents; that he was also fiery and strong. At the time now referred to, his fire was low and his strength distilled away; but the residue of his life was neither irritability nor discontent. He was unfit to mingle in society, for conversation was a pain to him; but let us observe the great man-child when alone. He is at the village of Interlachen, enjoying Jungfrau sunsets, and at times watching the Swiss nailers making their nails. He keeps a little journal, in which he describes the process of nail-making, and incidentally throws a luminous beam upon himself.

"Aug. 2d, 1841. Clout nail-making goes on here rather considerably, and is a very neat and pretty operation to observe. I

* See note, p. 195.

love a smith's shop, and anything relating to smithery. *My father was a smith.*

From Interlachen he went to the Falls of the Giessbach, on the pleasant Lake of Brientz. And here we have him watching the shoot of the cataract down its series of precipices. It is shattered into foam at the base of each, and tossed by its own recoil as water-dust through the air. The sun is at his back, shining on the drifting spray, and he thus describes and muses on what he sees:

"August 12th, 1841. To-day every fall was foaming from the abundance of water, and the current of wind brought down by it was in some places too strong to stand against. The sun shone brightly, and the rainbows seen from various points were very beautiful. One at the bottom of a fine but furious fall was very pleasant,—there it remained motionless, whilst the gusts and clouds of spray swept furiously across its place, and were dashed against the rock. It looked like a spirit strong in faith and steadfast in the midst of the storm of passions sweeping across it, and though it might fade and revive, still it held on to the rock, as in hope and giving hope. And the very drops, which in the whirlwind of their fury seemed as if they would carry all away, were made to revive it, and give it greater beauty."

ART. XVII.—*On Enargite from the Morning Star Mine, California;* by EDWARD W. ROOT, Assistant in the Laboratory of the School of Mines, Columbia College, New York.

IN November last I examined a copper ore from the Morning Star Mine, Mogul District, Alpine Co., California, which proved to be Enargite. This mineral occurs in a massive state, and also crystallized in small rhombic prisms, whose planes are much striated. These crystals are of a grayish black color, possess a very brilliant metallic luster and are about a millimeter in length. The massive mineral possesses a somewhat coppery color upon fresh fractures, while the exposed surface has a dark bluish tarnish. It is quite brittle. Streak black. Hardness about 4. Sp. grav. 4·34. B. B. decrepitates quite violently and fuses readily to a globule, giving off arsenical and sulphurous fumes and forming a coating of antimonous acid. With fluxes it gives the copper reactions. In a closed tube it fuses readily, giving off at a gentle heat a yellow sublimate of sulphur, and at an increased heat a reddish sublimate of tersulphid of arsenic. It is insoluble in hydrochloric acid. Soluble in nitric acid with

a residue of sulphur and antimonous acid. Soluble in aqua regia with a separation of sulphur.

Associated with the specimen submitted to analysis, was a little iron pyrites and quartz, while the magnet extracted a few small shining particles, which before the blowpipe gave the reactions for iron and titanic acid.

In the quantitative analysis of the mineral a special portion was taken for the sulphur determination; it was decomposed upon a waterbath with fuming nitric acid, which even in the presence of antimonous acid readily converted the sulphur into sulphuric acid, the solution evaporated to dryness, nitric acid expelled by hydrochloric, and the sulphuric acid precipitated as usual.

Another portion of the mineral was decomposed upon a waterbath with fuming nitric acid, evaporated to dryness, the residue dissolved in hydrochloric acid, the solution boiled with a concentrated solution of sulphurous acid in water, to reduce the arsenic to arsenious acid, and copper, arsenic and antimony precipitated as sulphids by hydrosulphuric acid. The moist sulphids were digested upon a waterbath with sulphid of potassium, which dissolved out the sulphids of arsenic and antimony. The residue, consisting of the sulphid of copper was dissolved in nitric acid, converted into the sulphate, and the copper precipitated in the metallic state by hypophosphite of magnesia, according to Gibbs's method.* The solution containing the arsenic and antimony as double sulphids with sulphid of potassium, was diluted with water, treated with a concentrated solution of sulphurous acid in water to an acid reaction, and boiled in a large glass flask for several hours. Both the sulphids of arsenic and antimony were at first precipitated, but upon boiling, the sulphid of arsenic was converted into arsenious acid. The sulphid of antimony was filtered off, an excess of sulphur removed by repeated treatment with bisulphid of carbon, converted into the antimoniate of antimony (SbO_3 SbO_5) by the action of fuming nitric acid, and estimated as such. The arsenic was precipitated after acidifying the solution with dilute hydrochloric acid, as the sulphid, by hydrochloric acid. The sulphid of arsenic was freed from an excess of sulphur by bisulphid of carbon, converted into arsenic acid by treating with fuming nitric acid upon a waterbath, and precipitated as usual by the magnesia mixture.†

* This Journal, II, xliv, 210.

† Bunsen, the originator of this separation of arsenic and antimony, has lately simplified the estimation of the arsenic, by converting the arsenious acid directly into arsenic acid, by passing a current of chlorine gas through the solution for several hours.

In two analyses the following results were obtained:

	1.	2.	Mean.
S,	31·81	31·51	31·66
Cu,	45·94	45·95	45·95
As,	13·65	13·74	13·70
Sb,	6·03	....	6·03
Fe,	0·81	0·64	0·72
SiO_2,	1·03	1·13	1·08
	99·27		99·14

In analysis No. 2, owing to accident, a part of the antimony was lost, but still some five per cent remained.

If the iron present is considered as iron pyrites, and this together with the silica deducted, the following mean is obtained:

S	Cu	As	Sb		
31·68	47·21	14·06	6·19	=	99·14

These numbers give the ratio 1 : 3 : 8 between the arsenic, copper and sulphur, corresponding to the formula $3Cu_2S+(As, Sb)S_5$, which is that of enargite as described by Breithaupt, Field and Burton.

This California enargite differs however from those heretofore described in having a much larger proportion of the arsenic replaced by antimony. It contains 6 per cent of antimony, while that from Chili analyzed by Field, contained none.* That from Peru analyzed by Plattner, 1·61 per cent.† That from New Granada, analyzed by Taylor, 1·29 per cent,‡ and that from Colorado, analyzed by Burton, 1·37 per cent.||

School of Mines, March 27, 1868.

Art. XVIII.—*Physical Observations on the Andes and the Amazons;* by James Orton.

The following observations were made during a scientific expedition across the continent of South America, in the year 1867. The instruments used were two mercurial barometers (one of them short, beginning to mark at 9,000 feet), a Wollaston boiling-point apparatus, and Boussingault's ground thermometer. They were constructed by Mr. James Green of New York, and furnished by the Smithsonian Institution. The barometers perfectly corresponded; and on returning to New York, it was found that the long barometer (its companion was broken in the valley of Quito,) after a tour of ten thousand miles had not varied a hair's breadth. The route

* This Journal, II, xxvii, 52.
† Pogg. Ann., lxxx, 383.
‡ Proc. Acad. Sci. Philad., 1857, 168.
|| This Journal, II, xlv, 34.

was from Guayaquil to Pará via Quito, Rio Napo and the Amazons. The chief value of these observations is derived from the fact that they were made with instruments of precision at many localities whose altitude was hitherto unknown, or obtained by an aneroid* or boiling apparatus. They also test the utility of the thermo-barometer, experiments having been made with it and the standard barometer simultaneously. It is doubtful if the two were ever carried together to such elevations and across the breadth of the continent. Indeed I do not know that any barometer has been taken down the Rio Napo. It was my desire to throw some light upon that strange anomaly in the Amazon valley first revealed by Lieut. Herndon and theorized upon by Lieut. Maury.† All the calculations of altitude are based upon the barometric observations with the Pacific level as a constant. On the Amazons and its tributary no correction is made for monthly variation as it is unknown; the experiments were made in November and December, and the correction would be therefore additive. The heights of points on the river are reduced to the level of the water; while the barometric pressures are given as they were taken, the elevation of the instruments varying from two to sixty feet. Thus at Guayaquil they were twenty feet above the river, at Napo, Coca and Pará, twenty-five feet, and at Pebas, sixty feet. The pressures corresponding to temperatures of boiling water are from Guyot's tables after Regnault revised by Moritz. These are placed along side of the barometric observations (reduced to 32°) in order to show the agreement between the barometer and boiling apparatus. To obtain the mean annual temperatures of important places, the ground thermometer was sunk from two to three feet, and allowed to remain about six hours.

The first desideratum was the level of the Pacific off Ecuador. After many careful calculations I fixed upon 29·930 as the barometric pressure at the freezing point. Herndon assumes 30·000, and his altitudes are therefore too high. I know not Humboldt's base, but his estimates are uniformly high. Those of Boussingault and Visse I have taken second-hand, and am not sure they are correctly copied. Pentland gives 29·944 as the mean barometer off Peru; Duperrey (1823) gives 29·961 at Paita; and Gilliss at sea off Cape Lorenzo, Oct. 2d, found 29·825. Pentland's corrected for latitude would very closely agree with mine.

The site of Quito is very uneven: my altitude is that of

* "A Traveler who carries an aneroid alone with him must not expect accuracy within two or three hundred feet."—Guyot. It is generally higher than the standard in high temperatures.

† See this Journal, vol. xix, p. 385.

the grand Plaza. It is a singular fact that La Condamine (1745), Humboldt (1802), Boussingault (1831), and Visse (1863) give a decreasing altitude. One is tempted to believe that the Andes are sinking. Boussingault contends that this is true of some individual mountains.* The mean of all the estimates given in the table, excepting that of Caldas which is manifestly incorrect, is 9,521. Villavicencio gives 9,485. I quote him simply to call attention to the fact that the estimates in his *Geografia de la Republica del Ecuador* are not reliable. In a balloon ascent made in January, 1864, from Woolwich, Eng., by Glaisher for the British Association, the reading of the barometer at 3^h 16^m P. M., (corrected and reduced to 32°,) was 20·951 at the estimated height of 9,500 ft. The minimum noticed in Quito was 21·460. The French savans give the length of the seconds pendulum at Quito 3·247753 ft., it being 3·250588 ft. at sea-level on the equator. This is a deduction, not the result of experiment, and gives only 9,166 ft. for the height of Quito. The pendulum experiment at Quito would be very interesting, but great disturbances would doubtless arise from the proximity of so many volcanic mountains.

The observations on Pichincha were taken 80 ft. below the highest pinnacle. That in the crater was made at the foot of the cone of eruption. That on Antisana was taken just above the average snow-limit; and that on Cotopaxi at the base of the cone. Cotopaxi's cone is therefore 6,000 ft. high. At Itulcache begins the series of observations from Quito eastward across the continent. The great Amazonian forest commences about ten miles west of Papallacta. Baeza is situated on a ridge; thence the path to Napo turns from an easterly course directly south through Archidona. Chinipleia was our camp on the Rio Cosanga. The heights on the Amazons as given by other travelers express the altitude above the Atlantic; while I have kept the Pacific as a base. The barometer and boiling point at the Atlantic level are computed; the distance of Pará being taken at 95 miles and the fall of the river two inches to the mile. This makes the Pacific off Ecuador about two feet higher than the Atlantic. Ought we not to expect a difference? Do not the great volcanic mountains on the equator exert an attractive power on the ocean at their feet? At Panamá, the Pacific and Atlantic sink to a common level, for there the Andes drop down to an insignificant altitude. I must add, however, that the obser-

* See Bull. de la Soc. Géol. de France, tom. vi, p. 56, Prof. Schön from observations at Wurtzburg thinks that the atmospheric pressure has increased during the last fifty years. Forbes, 1832.

vations at Guayaquil were taken in July, those at Pará in January with the same instruments. My barometric pressure at Pará may therefore be too high; but the true monthly variation is unknown. At Havana the mean barometer in January is ·200 in *excess* of the yearly mean, and the correction at the equator should be still greater; but Dewey makes the January variation at Pará ·011 *below* the mean. His mean in July is 30·020. We need an extended series of observations at Guayaquil and Pará. If we take Dewey's annual mean at Pará, 29·941 and 35 ft. as the altitude, the barometer at the Atlantic level would stand 29·977. But this makes the Atlantic full 40 ft. below the Pacific. Moreover, if Pará is 35 ft. above the Atlantic, the fall of the river is nearly 4½ inches per mile, which is also absurd. The rate cannot be far from two inches. This would make the fall about fifteen feet and the barometer at the Atlantic 29·932.

Slope and current of the Amazons.

	Distance.	Fall.
Napo village to the Marañon,	600 miles.	21·3 inches per mile.
" " " Pará,	2800 "	6·2 " "
Tabatinga " "	2000 "	2·5 " "

Azevedo and Pinto made the fall from Tabatinga to Pará, 0·9; Castlenau made it a little more. LaCondamine assigns to the Amazon in general a slope of 6·3 in. per mile, which very closely agrees with my calculation. Herndon, who, trusting his boiling apparatus, made Ega over 2,000 ft. high, deduced from this the descent of the Amazons "a little more than a foot per mile, which would about give it a current of 2½ miles per hour!"* He remarks that the current increases considerably after the junction of the Madeira. He calls it 2½ from Pebas to Ega in November, and three below Serpa in February. But the Peruvian navigators consider it from 3 to 3½ at Pebas in December (4 at high water). On the Napo in November I found it 5 below Coca; above Santa Rosa there are rapids. The Brazilian lieutenants make the slope of the Amazons exactly the same between Tabatinga and Manáos as between M. and Pará, or 58 ft. per 1,000 miles. It is interesting to compare the Amazons with other rivers:

Rhone,	fall	24·00 in. per mile;	slope,	1′ 18″
Mississippi,	"	19·87 "	"	1′ 4″
Thames,	"	17·5 "	"	0′ 57″
Nile,	"	6·5 "	"	0′ 21″
Amazons,	"	6·25 "	"	0′ 20″
Ganges,	"	4· "	"	0′ 13″
Ohio,	"	4· "	"	0′ 13″

* Herndon's calculations are very singular. Compare the one on page 258 with one on p. 331.

The Mississippi at St. Louis has nearly the same altitude* as the Amazons at the mouth of the Napo: respective distances from the sea, 1,400 and 2,200 miles. Below St. Louis the fall of the Mississippi is 3 in. per mile.

Accidental variations of the barometer.—These are generally considered as reduced to nothing at the equator. Humboldt says, the regularity of the ebb and flow of the aerial ocean is undisturbed by storms, hurricanes, rain and earthquakes in the torrid zone of the new continent, on the coast and at the elevation of 13,000 ft. At Dodabetta, India, (alt. 8,640 ft.) Lieut. Strachey found that the most violent and variable wind did not affect the range of the barometer. But in the valley of the Amazons strange irregularities have been noticed in every hypsometric instrument, whether mercurial barometer, aneroid or boiling apparatus. No two travelers have noticed the same irregularity; but all (save Azevedo and Pinto), have found a lawless disturbing force. Indeed, the anomaly is so constant, that I am inclined to suspect the record of the Brazilian lieutenants, because it discovers no irregularity. Spix and Martius gave Tabatinga the enormous altitude of 670 ft., and elevated Manáos 556 ft. above the sea. This makes the fall of the first thousand miles of the river five times that of the second. Castlenau found Nauta 365 ft., Pebas, 399 ft., and Manáos 293 ft. above the sea; and complaining that his barometer got out of order, rejected a part of his observations. Herndon, with a boiling apparatus, discovered to his surprise that between Nauta and Ega he was ascending according to the instrument, though by the river and his own senses he was descending. At Nauta water boiled at 211°·3, and at Ega at 208°·2, which would put Nauta about 400 ft. above the Atlantic, and Ega, 800 miles farther down stream, at 2,000 ft.! On the Rio Purus, Chandless' barometer stood higher (29·871) than at Manáos, while the observations of Spruce and Wallace, one with an aneroid, the other with a boiling apparatus, made Manáos *lower* than Pará.

My experience was similar. The barometric pressure was very orderly in its increase from the top of Pichincha down to Pará, excepting on the Napo where at one time both the barometer and boiling apparatus unanimously declared that our canoes were gliding up stream though we were descending at the rate of five miles an hour, and excepting also on the Amazons between Tabatinga and Obidos. The boiling point was more irregular than the height of the mercury. Our rate down the Napo was full 40 miles a day; and the record of the boiling

* Mean bar. at St. Louis, 29·520; at the mouth of the Napo, 29·526.

point for five successive days below the Curaray is as follows: 210°·8; 211°·1; 211°·; 210°·9; 211°·4.

Similar anomalies have been noticed in other parts of the world. Erman's observations in Siberia would place Jakuzk below the level of the sea of Ockozk; yet the Lena flows down from Jakuzk to the sea. Von Buch observed that the mean pressure on the shores of the Baltic was less than in France, and imputed the difference to what he calls a *vallé atmospherique.* The barometric mean at Great St. Bernard deviates by 14 ft. from the true height ascertained by leveling; this Plantamour attributes to an abnormal depression of the temperature of Geneva owing to the neighborhood of the lake. The barometer makes the Caspian Sea 250 ft. higher than the level; and the Antarctic Ocean, 800 ft. above the Atlantic.

The cause of the strange phenomena in the Amazon valley, Herndon (and after him Maury) ascribed to the pressure of the trade-winds as they are dammed up by the Andes. This gigantic wall of mountains, regarded as the rocky shore of an aerial ocean, is equivalent to 14 in. of the barometric column or ½ the weight of the atmosphere. But this theory is not wholly satisfactory. It is true that the inclination of the continent to the east would naturally give a higher barometer than if it were a level table land. "When the atmosphere is swept *en masse* from the sea up the gradual slope of the land surface, we must expect (says Herschel) great discordances between barometric and trigonometric determinations of altitudes." But if the banking up of the trade-winds is the sole cause of the perturbations, ought we not to expect either a gradual though inordinate increase of the barometric pressure in ascending the Amazons, or fixed points of maximum intensity; yet the fluctuations seem to follow no rule. Herndon found the greatest disturbance above Tabatinga; I found it below. Herndon made Ega 2,000 ft. above the sea; I found it 100. Compare also the various altitudes given to Manáos. It is singular that in Herndon's Meteorological Journal, we find a light breeze, *seldom eastward,* between the Huallaga and Ega, while between Ega and Manáos the prevailing wind is east and stronger. The wind theory would lead us to expect just the reverse. It cannot be said that the difference in the observations is due to the fact that they were taken at different seasons of the year. Herndon and myself were at Ega in the same month (December); he boiled water at 208°·2; I had to raise it to 211°·9. I was at Pebas just a month later than Herndon; yet we found the boiling point exactly the same. The trade-winds doubtless have something

to do with these variations, as also the position of the moon.* But may not *one* disturbing force be found in the presence of the great Amazonian forest which is a powerful condenser of aqueous vapor? The vapory winds, fresh from the Atlantic, would give a low barometer; after condensation, the mercury would rise.

Overlooking the irregularities between Tabatinga and Obidos, I think my barometrical observations across the continent faithfully delineate the main features from Guayaquil to Pará. We see a striking difference between South America and our own continent in the fact that while Tabatinga, the half-way station, is not over 255 ft. high, Fort Leavenworth at the same distance from the sea, is six hundred feet above it. I will here notice a curious coincidence (it can be nothing more) in the relation between the eastern profile of a continent and its section. Thus, the eastern coast line of the American continents is a rough copy of the line describing the surface from west to east. In the eastern hemisphere, the eastern contour more nearly approximates a section from north to south; and there is some reason for this, for the ocean would eat most deeply into the land between the mountain chains. In the case of South America, the protuberance of Cape St. Roque represents the swell of the Andes; the slope thence to Patagonia is an imitation of the Amazonian valley; while the upward turn of the Tierra del Fuego reminds us of the low Brazilian mountains.

Diurnal variations of the barometer.—These occur with great regularity, notwithstanding violent winds, sudden changes of temperature and humidity. The configuration of the land in India greatly modify the atmospheric tides; but at the foot of the Andes the variations attain their extreme limits at the same hours (10 A. M. and 4 P. M.) as in the high valleys.† The hour of the day may be determined by the barometer at Quito within 15 minutes. The mean daily amplitude,

At Guayaquil is	·091	(Visse, ·147; Darondeau, ·145.)
" Quito,	·084	(Visse, ·090.)
" Pebas,	·104	
" Pará,	·088	

* Sabine found at St. Helena that the mercury was ·004 in. higher when the moon was crossing the meridian above and below the horizon.

† The daily tides were not observed by Richardson at Gt. Bear Lake, lat. 66° 54′; while Prof. Forbes asserts that the barometer at St. Bernard (8,000 ft. high) is lowest at 9 A. M. and highest at 3 P. M; and that Capt. Parry noticed the same reversion in lat. 74°. Dr. Hayes found the maximum at Port Foulke 6½ P. M. and minimum, 3 A. M.

The extreme range decreases with the latitude and also with the increase of altitude:

At Port Foulke, lat.	78° 18′ N.	it is	1·81 in.	(Sonntag.)
" Pará,	1° 30′ S.	"	·30 "	(Wallace.)
" Guayaquil,	2° 12′ S.	"	·166 "	(Orton.)
" Quito,	0° 13′ S.	"	·130 "	(Orton; LaCondamine, ·133.)
" Antisana Hacienda,	0° 30′ S.	"	·065 "	(Aguirre.)

The diurnal variation decreases with the increase of altitude as well as with the increase of latitude:

Increase of elevation—

Guayaquil,	alt.	10 ft.	var.	·091	
Pará,	"	15	"	·088	
Quito,	"	9500	"	·084	
Antisana,	"	13,300	"	·022	(Aguirre.)
Anomalous, Pebas,	"	345	"	·104	
" Manáos,	"	120	"	·111	(Spruce.)

Increase of latitude—

Guayaquil,	lat.	2° 12′ S.	var.	·091	
	"	23° 55′ N.	"	·060	(computed by Kaemtz.)
	"	39° 4′ N.	"	·044	" " "
Arctic Sea,	"	72° 15′ N.	"	·038	(Sonntag.)
"	"	78° 28′ N.	"	·013	"
"	"	81° 30′ N.	"	·001	"

Increase of altitude with increase of latitude—

Paris,	lat.	48° 50′	alt.	200 ft.	var.	·0315
Geneva,	"	46° 12′	"	350 "	"	·0354
Faulhorn,	"	46° 30′	"	9000 "	"	·0106

Max. fluctuation	at Port Foulke is in	April;	minimum,	October.
"	"	Philadelphia,	Jan.;	"	May.
"	"	Antisana,	June;	"	December.

Throughout Asia the barometric mean is less in summer than in winter; in Pará, Antisana and Sitka it is the reverse. The monthly variation decreases from the poles to about latitude 40°; thence to the equator it increases. At the equator, (I know not how it is in high latitudes) it decreases with the altitude.

The annual range at Port Foulke (0·4 in.) is about 20 times the diurnal; that of Antisana (0·05) is about twice the diurnal. The annual variation is more decidedly marked in India than in tropical America. This is owing, says Dove, to the comparatively constant wind throughout the year in the New World; while in India, equatorial currents prevail when the sun's altitude is greatest and polar currents when it is least.

Comparison of mercurial barometer with boiling apparatus.—After numerous observations taken with these instruments side by side from the sea-level to the top of Pichincha, I conclude that the boiling apparatus, though very convenient, can not be depended upon and is the least reliable in high altitudes. In the Amazonian valley it is certainly useless. The accuracy of Regnault's Tables of corresponding pressures was tested on the Andes by Visse in 1848, and he found the agreement very satisfactory. But my experiments on the same ground do not confirm this. At Guayaquil he found a difference of ·0092 in.; I found ·008. At Quito his difference was ·015 ; mine ·045. On Pichincha, his difference was ·0045 ; mine, taken at the same hour of the day, was ·008. At Papallacta, the boiling point indicated a pressure ·205 too small, and at Archidona a pressure ·364 too great. On Mont Blanc, Saussure found the barometer 17·136, and the pressure indicated by boiling water, 17·883. At Rochester, they differ by ·130.

Mean annual temperature.

Guayaquil, 83° (Boussingault with ground thermometer sunk one foot found 78°·8; Hall with six months obs. with air thermometer found 78°·03. Appleton's Am. Cyclop. gives 88°.)

Quito, 58°·8 (LaCondamine, 58°; Humboldt, 57°·92; Boussingault, with ground ther. sunk one foot, 59°·36; Caldas, 59°·; Hall and Salaza, 59°·6; Aguirre, 58°·1.)

Archidona,	77°·	
Santa Rosa,	79°·5	
Pebas,	80°·	(Castelnau, 79°·7)
Tabatinga,	82°·	(" 79°·34)
Pará,	80°·2	(Dewey, 80°·5)

The isothermal line in the United States corresponding to the mean temperature of the valley of Quito runs through the state of Tennessee. The temperature of Nashville is 58°·5. The mean temperature at Fort Massachusetts on the Rocky Mountain plateau (alt. 8400 ft.) is 41°·1. The temperature of Quito is just 1° less than that of Rome; but the spring months at the two cities have the same mean temperature. The mean temperatures of Antisana Hacienda and Quebec are the same, but the seasons are nearly reversed.

	Spring.	Summer.	Autumn.	Winter.	Year.
Quebec,	38°·00	67°·67	43°·67	13°·33	40°·67
Antisana,	42°·16	38°·26	40°·70	41°·80	40°·70

The coldest hour at Quito is 6 A. M.; the warmest between 2 and 3 P. M. The latter corresponds with that on the east coasts of the United States and Asia; on the west coast of America and Europe it is 1 or 1½ P. M. The greatest heat at Pará occurs at 2 P. M.; the hottest month is November and the

coolest is March. The decrement of temperature in ascending from Geneva to St. Bernard is 1° for every 487 ft.; between Quito and Antisana it is 1° for every 340 ft.

Barometrical measurements across South America.

Locality.	Alt.	Barom.	Boiling Point.	Regnault's Equiv.	Difference.	Other estimates.
			°			
Pacific Ocean,	0	29·930	212·01			*Bar*. of Visse, 29·904; Boussingault, 29·867.
Guayaquil,	10	29·899	211·95	29·891	—·008	*B. P.* of Visse, 211°·8.
Guaranda,	8840	21·976				*Alt.* of Visse, 8,872; Hall, 8,928.
Arenal,	14,250	18·123				*Alt.* of Visse, 13,917; Hall, 14,268.
Mocha,	10,900	20·393				
Ambato,	8490	22·241				*Alt.* of Visse, 8,541; Boussingault, 8,787. *Bar.* of Jameson, 22,218.
Tacunga,	9181	21·693				*Alt.* of Visse, 9,180; Boussingault, 9,384. *Bar.* of Jameson, 21·700.
Tiupullo,	11,662	19·858				*Alt.* of Visse, 11,702.
Machachi,	9900	21·212				*Alt.* of Visse, 9,823.
Quito,	9520	21·530	195·8	21·485	—·045	*Alt.* of LaCondamine, 9,596; Humboldt, 9,570; Caldas, 8,947; Boussingault, 9567; Visse, 9307; Aguilar, 9496; Bureau des longs. 9540; Tramblay's *Ann.* 9538; Jameson, 9513. *Bar.* of LaCondamine, 21,404; Humboldt, 21,403; Aguilar, 21·465; Jameson, 21·566. *B. P.* of Tramblay, 184°·18; Visse, 195°·6.
Panecillo,	10,101	21·043				*Alt.* of Humboldt, 10,244; Aguilar, 10,135. *Bar.* of Jameson, 21·207. *B. P.* of Visse, 194°·7.
Pichincha, top.	15,827	17·038	184·5	17·030	—·008	*Alt.* of LaCondamine, 15,606; Humboldt, 15,922; Visse, 16,200; Hall, 15,380; Boussingault, 15,676; Jameson, 15,704. *Bar.* of Visse, 16·942.
Pichincha, crater.	13,800		189·2	18·672		*Alt.* of Visse and Moreno, 13,600.
Antisana H.	13,300	18·583				*Alt.* of Humboldt, 13,465; Boussingault, 13,356. *Bar.* of Jameson, 18·630; Aguirre, 18·573.
On Antisana,	16,000	16·782				
Pinatura,	10,410	20·791				*Alt.* of Boussingault, 10,348.
Padregal,	11,860	19·817				
On Cotopaxi,	12,860	19·004				
Riobamba,	9200	21·705				*Alt.* of Boussingault, 9413; Visse, 9157.
Cajabamba,	10,918	20·512				*Alt.* of LaCondamine, 11,000.
Itulcache,	8885	22·006				
Tablon,	10,516	20·800				
Papallacta,	10,511	20·803	193·8	20·598	—·205	

Locality.	Alt.	Barom.	Boiling Point.	Regnault's Equiv.	Difference.	Other estimates.
Guña,	8622	22·206				
Pachamama,	7920	22·751				
Baeza,	6625	23·793				
Chinipleia,	6200	24·145				
Cochachimbamba,	4252	25·832				
Curi-urcu,	3247	26·746	°			
Archidona,	2115	27·816	209·00	28·180	+·364	
Napo,	1450	28·419	209·4	28·407	−·012	
Santa Rosa,	1100	28·814	210·4	28·982	+·168	
Coca,	858	29·022	210·65	29·127	+·105	
M'h of R. Aguarico	586	29·321	211·00	29·331	+·010	
do. R. Curaray,	500	29·408	210·8	29·215	−·193	
do. R. Napo,	385	29·526	211·4	29·566	+·040	*Alt.* at Nauta by Castelnau, 365.
Pebas,	345	29·510	211·1	29·390	−·120	*Alt.* of Herndon, 537. *B. P.* of Herndon, 211°·1.
Loreto,			211·4	29·566		
San Antonio,	256	29·655				
Tabatinga,	255	29·656	211·5	29·625	−·041	*Alt.* of Spix and Martius 670; Azevedo and Pinto, 150; Agassiz, 200.
Tunantins,	138?	29·770				*Alt.* of Azevedo and Pinto, 124.
Ega,	100?	29·813	211·9	29·862	+·049	*Alt.* of Herndon, 2052; Azevedo and Pinto, 120; *B. P.* of Herndon, 208°·2.
Manáos,	199?	29·705				*Alt.* of Herndon, 1475; Castelnau, 293; Spix and Martius, 556; Azevedo and Pinto, 92. *B. P.* of Herndon, 209°·3; Gibbon, 210°·87; Wallace, 212°·5.
Serpa,	158?	29·752				*Alt.* of Azevedo and Pinto, 84.
Obidos,	114	29·802				*Alt.* of Azevedo and Pinto, 58; Agassiz, 45.
Santarem,	107	29·808	211·5	29·625	−·183	*Alt.* of Herndon, 846; Azevedo and Pinto, 50. *B. P.* of Herndon, 210°·5.
Mt. Alegre,	83	29·834				
Gurupá,	38	29·890				*Alt.* of Azevedo and Pinto, 42.
Pará,	15	29·889	211·95	29·891	+·002	*Alt.* of Herndon, 320; Azevedo and Pinto, 35; Dewey, 35. *Bar.* of Herndon, 29·708; Dewey, 29·941; Orton (reduced to level of river) 29·914. *B. P.* of Herndon, 211°·5.
Atlantic Ocean,	−2	29·932	212·16			*Bar.* of Dewey, 29·977.

Rochester, N. Y., July 15, 1868.

Art. XIX.—*Notes on the Caucasus;* by Capt. F. von Koschkull. (Communication addressed to Prof. J. S. Newberry, and translated by him for this Journal.)

The interest in science manifested by you, led me to communicate to you some facts concerning the Isthmus of the Caucasus; but not satisfied with a verbal recital, you have requested me to make the same communications in writing. Your cordial hospitality and constant assistance to me, a stranger, compel me to comply with your request even under the unfavorable circumstances in which I am placed, for at present with the exception of the little map of the Caucasus lately published by Petermann, I have neither notes nor books of reference by me to enable me to verify the statements I intend to present to you in this report, and I am therefore obliged to rely entirely upon my memory. You are well aware, moreover, that there are many things easy to be communicated by word of mouth, but difficult to express in writing, especially, when one is compelled to trust to memory alone. For this I beg the indulgence, not only of yourself, but of every one who may read this article, that you will not expect precise accuracy, but accept it as a general description of the Isthmus of the Caucasus, prepared with the sole object of showing my willingness to gratify any wish of those toward whom I entertain feelings of the deepest gratitude.

As I propose to speak also of the geology of the Caucasus, I cannot pass by without notice the name of Mr. Abich, since we owe our increased knowledge of the country under consideration entirely to the labors of this celebrated and indefatigable savant who for some time past has devoted himself to the study of this region.

Any communications that I may make concerning the biology and ethnography of this country must be very brief, partly for want of the necessary notes, and partly because, with reference to these subjects, the Caucasus has not, up to this time, been sufficiently explored, owing to the constant wars between Russia and the mountaineers. But since the termination of hostilities in 1864, a scientific exploration of this region has been conducted by Mr. Raddé, the naturalist, and it is to be hoped that the labors of this young and enterprising *savant* will be as successful as those which he prosecuted in the eastern portion of Siberia.

If in this short description there are points which are not quite clear or exact, I trust it will be attributed only to my defective memory, and I once more ask for indulgence from my readers for the reasons which I have stated above.

The country which separates the Black sea and the sea of Azoff from the Caspian, and at the same time connects the south of Russia with Asia Minor and the northern part of Persia, is generally called the Isthmus of the Caucasus. It lies between the parallels of 39° and 46° north latitude, and between the meridians of 54° and 68° east of Greenwich. This region resembles an oblique parallelogram, of which the longitudinal diagonal runs from northwest to southeast, and is about 1200 kilometers long. The transverse diagonal runs from northeast to southwest and its length is about 625 kilometers. All of this country is traversed by ranges of mountains called the Mountains of the Caucasus, and from them the name has passed to the whole Isthmus.

The internal forces of the earth which elevated these mountain chains were exerted in several directions, forming what must be classified as *principal* and *subordinate* lines of upheaval. The directions of the principal lines are, 1st, from north-northwest to south-southeast. This is the trend of the great chain of the Caucasus of which the extremities form the peninsula of Taman on the north-west and Apscheron on the southeast, the latter extending into the Caspian sea. A line of elevation having the same direction in the southern part of the Isthmus is known as the Little Caucasus.

Second direction, from north-northeast to south-southwest. This line is marked by the chain of mountains which bears the name of Karthlo-Imeritia and connects the principal chain of the Isthmus with the Little Caucasus between 41° 40′ and 42° 40′ north latitude, and 60° 40′ and 61° 40′ east longitude. Axes of elevation having this trend are found in the southeast part of the principal chain of the Caucasus in what is known as the country of Daghestan, and also in the Little Caucasus.

The subordinate lines of elevation are first north and south, as best seen on the plateau north of Mt. Elbruz, forming the watershed between the Caspian and the Black sea. Axes with this trend are also seen in Daghestan and the Little Caucasus. Second, east and west. Ranges having this bearing are seen in the mountains of Abkhazi, Swanethi and Imeritia, the southern ramifications of the Grand Caucasus; also in the chain of Trialethi which extends from the Karthlo-Imeritia mountains eastward to the city of Tiflis; and finally the trend is observable in the inferior ranges of Daghestan and the Little Caucasus.

A knowledge of these bearings is not only indispensable to a correct understanding of the structure of the Caucasus, but enables one to comprehend the relations which this great mountain system bears to those on the east and west of the Isthmus. Following the direction of the principal range of the Caucasus

from the peninsula of Apscheron, a submarine elevation can be traced extending southeasterly. The island of Tschelekan is on this line, and its prolongation forms the axis which connects with the mountains of Central Asia.

On the peninsula of Taman, formed by the northwest extremity of the main chain of the Caucasus, is seen the intersection of two lines of upheaval; one, the principal line, running from southeast to northwest, and the other, that of the mountains of the Crimea, from southwest to northeast. The intersection of these two lines mainly determines the configuration of the Isthmus. The northeast and southwest upheaval has not only united the principal chain with the Little Caucasus but has formed a juncture between the mountains of the Isthmus and those of Persia and Asia Minor.

The points of intersection of the main lines are of considerable altitude, as, for example, Sikara, where the principal range crosses the mountains of Karthlo-Imeritia, and Ararat, at the junction of the Persian mountains and those of the Little Caucasus.

The following table gives the position and altitude of the most important peaks of the Isthmus on a northwest and southeast line.*

	North Latitude.		East Longitude.		Height in feet.
	°	′	°	′	
Oschten,	44		57	40	
Fischt,	43	50	57	40	
Schougousch,	43	45	57	55	
Elbruz,	43	20	60		17,425
Dykh-tau,	43		60	40	16,857
Aday-khokh,	42	45	61	30	14,303
Kion-khokh,	42	55	61	40	10,536
Sikara,	42	35	61	40	11,794
Kazbek,	42	40	62	10	15,524
Teboulos-mtha,	42	35	63	15	13,869
Borbalo,	42	30	63	15	9,547
Dyklos-mtha,	42	30	63	35	12,838
Murou-dagh,	41	45	64	10	10,427
Sary-dagh,	41	50	64	20	11,268
Djulty-dagh,	42		64	30	11,668
Alakhoun-dagh,	41	50	64	40	11,912
Sawalat-dagh,	41	25	64	40	11,439
Schalbouz-dagh,	41	20	65		12,834
Schah-dagh,	41	15	65	15	13,090
Bazarduzi,	41	10	65	20	13,814
Dybrar,	40	55	66	20	6,804

* The longitudes are reckoned from Ferro and the heights (it is presumed) are in Paris feet.—EDITORS.

	North Latitude.	East Longitude.	Height in feet.
In the mountains of the Little Caucasus,	° ′	° ′	
Alagöz,	40 35	61 25	12,606
Great Ararat,	39 40	62	15,871
Little Ararat,	39 40	62 5	12,056
Kapoutschi-dagh	39 15	63 45	12,061
The most remarkable passes are,			
In the main range northwest of the Oschten mountain,			
Goitkh,	-----	-----	2,300
Pschekh,	-----	-----	5,100
Between the Mts. Fischt and Schougousch,			
Schetlib,	-----	-----	5,600
Between the Mts. Schougousch and Elbruz,			
Psegaschka,	-----	-----	5,809
Sant scharo,	-----	-----	7,500
Maroukh,	-----	-----	10,790
Between the Mts. Kazbek and Teboulos-mtha,			
Goudaour,	-----	-----	7,362
In the mountains of the Little Caucasus,			
Near Lake Goktschai,	40 10	62 41	6,000

Since the predominant trend northwest of the Elbruz is from north-northwest to south-southeast, this portion of the Isthmus range is composed of several distinct chains parallel to each other. Between the Elbruz and the Kazbek, the intervention of a northeast and southwest upheaval causes the regularity to disappear. In Daghestan, southeast of the Teboulos-mtha, there are no regular lines of upheaval; the interior forces of the earth having acted in different directions, and thrown the mountains into irregular and entangled masses. The same confusion is found in the Little Caucasus, though the general altitude of the mountains is less than in Daghestan.

Between the Schougousch and Bazaduz mountains, the main chain of the Caucasus presents an unbroken belt of perpetual snow; nevertheless the loftiest portion of the whole Caucasian range is between the mountains of Elbruz and Teboulos-mtha, and here are found the most remarkable glaciers of the Isthmus. Toward the peninsulas of Taman and Apscheron, the altitudes fall off to within a few hundred feet above the level of the sea. In the Little Caucasus there are also snow-covered summits, but they are scattered about and do not present an unbroken belt of snow like those of the principal range.

The formation of the valleys is determined altogether by the arrangement of the mountains. West of the Teboulos-mtha

mountain there are longitudinal valleys, the most important of which are between the Elbruz and the Kazbek; the two valleys of Swanethi on the upper parts of the rivers Ingur and Tzkhlenitz-Tskhale, the valley of Radscha on the Rion, and generally those inhabited by the Ossethes in the midst of the main chain. In Daghestan and the Little Caucasus, there is no regularity in their form or distribution.

The principal range is bounded on the north and south by vast plains running parallel to the mountains, that is, north-northwest and south-southeast. The northern plain merges insensibly into the southern prairies of Russia, the southern serve to separate the two Isthmus ranges.

By the north and south and north-northeast and south-southwest upheavals they are almost equally partitioned. The plateau north of the Elbruz forms, on the northern plain, an eastern division, along the valley of the river Terek, which belongs to the basin of the Caspian sea, and a western division belonging to the Black sea.

The southern plain is divided by the Karthlo-Imeritia mountains into the valley of the Kur, in the Caspian basin, and into that of the river Rion, in the Black sea basin.

These two plains may be considered as the remains of straits which united the Black sea and the sea of Azoff to the Caspian. The upheaval of the Karthlo-Imeritia mountains and the plateau north of the Elbruz mainly caused the separation of these two basins which now show a very considerable difference of level; for the surface of the Caspian is nearly 80 feet below the level of the Black sea, and the level of the latter must be looked for, some distance from the mouths of the rivers Terek and Kouban.

The sedimentary formations of the Isthmus of the Caucasus belong to the Jurassic, Cretaceous and Tertiary periods. With the exception of certain Tertiary beds, none of these strata are rich in fossils, but a sufficient number have been found to establish the fact that we here have representatives of the European deposits indicated below. The Jurassic of the Caucasus corresponds to that of Germany, and, as a consequence, includes the equivalents of the Oolite and Lias, both of which are largely developed. The Wealden is wanting; the Cretaceous strata belong to the epochs of the Neocomian, the greensand or gault and white chalk. The Tertiary strata may be referred to the Eocene, Miocene and Pliocene epochs, all of which are rich in fossils. Among the igneous and metamorphic rocks are granite, gneiss, feldspathic porphyry, diabase, melaphyre, basalt, diorite, trachyte, trachytic tufa, obsidian and phonolite. To these should be added a group of metamorphic schists, viz., chloritic, talcose, and argillaceous. These latter are most abundant in

the central part of the principal chain where they form the serrated crest. The same rocks are also found in the Little Caucasus, but not in great force. From the want of fossils, it is impossible to say whether these metamorphic slates belong to the Liassic formation or one still more ancient.

Granite and gneiss are found upon the northern slope of the principal chain between Elbruz and Kazbek, in the valleys of the tributaries of the Terek. Over all this region the granite occupies the lowest place of all the rocks visible, and, judging from the observations already made, it would seem that the granite masses in the region indicated the base of the principal mountain chain, associated with which are pyroxenic and trachytic rocks, which have been the active agents in the elevation of the metamorphic rocks.

On the southern slope, granite appears at one point in the chain of Karthlo-Imeritia, and here also occupies the lowest position.

The pyroxenic rocks, as melaphyre, pyroxenite, porphyry, basalt, and all the varieties of feldspathic porphyry, make their appearance in the different parts of the principal chain of the Caucasus. These rocks are not only the agents of elevation of the main chain, but they also form the most elevated summits between Fischt and Teboulos-mtha.

At the northwest of Fischt, in Daghestan, all the igneous rocks are covered by sedimentary deposits of enormous thickness, and such as have suffered great changes. In the southeast portion of the Little Caucasus, known under the name of the mountains of Karabagh, amphibolic rocks, diorite and syenite, are found, but the principal agents in the formation of the Little Caucasus would seem to have been trachytic rocks, such as trachytic, trachyte tufa, obsidian, and phonolite. In the western part of this chain, the rock and strata form great plateaus, which are sometimes deeply cut by fissures, in which the draining streams flow. There are also here many craterform depressions, which are now occupied by water, forming a series of làkes. The most remarkable of the plateaus are those of Gori and Alexandropol, which have an altitude of 4500 feet above the level of the sea, and the plateaus of Akhalkalaki and Bajazette, which are 5000 to 6000 feet above the sea level.

The largest of the lakes to which I have referred are Toporavan, at an elevation of 5600, and Goktschai, 5925 feet. The latter is 70 kilometers long, and 35 kilometers wide.

In the Little Caucasus the trachytic rocks have not only contributed to form the plateaus, but compose many of the conical summits, as Mt. Ararat and Mt. Alagöz. The trachytes are far less abundant in the main chain of the Caucasus, being there limited to the axis of elevation on which Mt. Elbruz is situated.

The sedimentary strata which covers the igneous and crystalline rocks, and which enter so largely into the composition of the mountain chains of the Caucasian isthmus, belong to the Jurassic and Cretaceous formation.

The Tertiary strata are spread over the plains bordering the Great Caucasian chain, both on the north and south, the plains traversed by the rivers Terek and Kuban on the north, the Rion, Kur and Araxes on the south.

From the northwestern extremity of the main chain of the Caucasus, as far east as Mt. Elbruz, the portion of the stratified rocks is quite regular. They here lie in a series of parallel folds, which have a northeasterly trend. It results from the regularity in the position of the strata, that on going from the main chain toward the north, each new element in the geological series represents a more recent, while on going south from the axis, more and more ancient strata are successively passed over.

South and east of Mt. Elbruz, the relations of the different strata become very complicated and obscure, from the intersection of various lines of elevation. In Daghestan and the Little Caucasus, this irregularity in the position of the sedimentary rocks is particularly marked.

In the Tertiary deposits, the Eocene and Miocene beds exhibit some of the undulations which characterize the underlying rocks ; but the Pliocene and Arabo-Caspian deposits are almost always quite horizontal.

The line of contact of the amphibolic and pyroxenic rocks, with the other igneous rocks, as well as with the sedimentary strata, is almost always, throughout the Caucasus, marked by the presence of metallic ores. As regards the number of mineral deposits known and worked, the Little Caucasus should be given the first place.

Argentiferous galena has been found in nearly all the mountainous portions of the Caucasian Isthmus. The inhabitants of these districts, warlike in character and devoted to the chase, have always needed lead with which to form their bullets, and from this cause the veins of this mineral have not only been sought and discovered, but in many places they have been extensively worked. To obtain metallic lead from the ore, this latter is melted in small furnaces, in a very rude way, the silver which it contains being neglected.

The most important veins of lead are found on the northern slope of the main chain of the Caucasus, between Mt. Elbruz and Mt. Kazbek, in the valleys of the rivers Ard and Baksan, tributaries of the Terek. Here the galena is accompanied by blende, copper and iron pyrites, in veins cutting granite, diorite, and metamorphic slates.

The mountaineers have had knowledge of these veins for a very long time, and have worked them somewhat, but in a very primitive manner. Ten years since, these deposits were purchased by the Russian government, for the purpose of introducing their improved methods of mining and reduction. After some years spent in explorations, the mines were regularly opened, and smelting works established, which have, up to the present time, produced about 1600 kilograms of silver, and 1,600,000 kilograms of lead. In the Little Caucasus there are veins of argentiferous galena, which were worked in ancient times by the Greeks of Asia Minor, who were able not only to obtain the lead, but also to separate the silver from it. Since the commencement of the present century, these mines have been entirely abandoned by the Greeks, probably from the impossibility of following the veins to great depths, with their imperfect system of mining.

Copper is also found in the Great Caucasus, but in much the larger quantity in the mountains of the Little Caucasus. For the most part, the ores of copper found there are sulphids which occur in veins in erupted masses of diorite. Copper has been mined on the Caucasus from the remotest antiquity. The process employed, both of mining and heating the ores, were of the simplest character, and such as belong to the infancy of the art. The productions of the copper-bearing districts is doubtless due to the richness and abundance of the ore. The average richness is from 10 per cent to 15 per cent metallic copper. The annual production of copper is at present about 156,000 kilograms. In the mines of copper, ores of cobalt have been found within the past two years. These are not treated at home, but are exported to Saxony in large quantities.

The ores of Iron, as specular ore, are found on the southern slope of the principal chain of the Caucasus, on the upper part of the valley of the river Rion, and also in great quantity on the northern slope of the Little Caucasus. Generally these ores are found in veins which accompany erupted masses of porphyry. They have been worked for ages in these two localities, but in a very rude manner. The iron is produced in small Catalan forges, and in blooms that are subsequently farther worked in a kind of blacksmith's forge. Naturally, the production of iron by this process would not suffice for the supply of the country; and the wants of the indispensable metal is mainly met by importations from the furnaces of the Urals. Only within the last five years, have properly built and efficient iron works been established in the country under consideration.

[To be continued.]

ART. XX.—*On some points in the Geology of Vermont;* by T. STERRY HUNT, F.R.S., of the Geological Survey of Canada.

(Read before the meeting of the American Association at Chicago, August, 1868.)

IT is proposed in the present communication, to state some facts with regard to the distribution of the paleozoic rocks of western Vermont, but before doing so it will be well to recall certain points in the geology of the province of Quebec. In this province, to the southeast of the St. Lawrence, there is displayed a great series of rocks to which Sir William Logan has given the name of the Quebec group. In the geology of Canada, published in 1863, this group was divided into two parts, an upper called the Sillery, and a lower the Levis formation, while some black limestones and shales occurring at the base of the latter were referred, doubtfully, to the Potsdam formation. These have, however, since been found to be paleontologically connected with the lower and fossiliferous parts of the Levis formation, as seen in the section on the island of Orleans near Quebec, the whole forming a natural division, which includes 4680 feet at Phillipsburg with the addition of the lower 1285 feet of the Orleans section, where the inferior measures of the group, seen at Phillipsburg, are not brought into view. To this portion of the Quebec group it has been found convenient to restrict the name of the Levis formation, while the succeeding part of what was at first called by that name, extending upward to the base of the Sillery, is distinguished as the Lauzon formation.

The entire thickness of the Levis, so far as known, is over 6,000 feet, and that of the Sillery about 2,000, while the Lauzon is extremely variable, ranging from a few hundred to more than 2,000 feet in thickness.

This middle division is characterized by two bands of magnesian and metalliferous rocks, one near its base, and the other at its summit. The latter band, for convenience of definition, has been united with the succeeding division or formation, and is represented on the geological map as forming the base of the Sillery. These two magnesian bands are of considerable economic importance, and contain interstratified deposits of copper, iron and chrome ores, together with dolomite, magnesite, steatite, serpentine, diorite, chloritic and epidotic rocks, associated with reddish and greenish shales and sandstones. The fauna of the upper divisions of the Quebec group is as yet limited to an *Obolella* and two species of *Lingula* in the Sillery, but the

Levis division has yielded a great number of organic forms which have been carefully studied by Mr. Billings and Prof. James Hall. The latter has described not less than fifty-one species of Graptolitideæ, which are figured in the second decade of Canadian Fossils, while most of the other species, including twenty-eight brachiopods, forty-two gasteropods, twenty cephalopods, and seventy-four crustaceans, have been described by Mr. Billings. The whole number of species described up to this time from the Levis formation, is 219; of these five have been detected in the Chazy and ten in the Calciferous, all the others being peculiar to the Quebec group, whose position is thus, it would seem, clearly defined as intermediate between the Calciferous and Chazy formations; none of its numerous species having been recognized in higher or lower rocks than these. The chief localities of these fossils are at Point Levis and Orleans island near the city of Quebec, and at Phillipsburg and Farnham near Missisquoi bay; but many of the characteristic forms are met with at numerous points between these two regions, the graptolites of the division in particular having been recognized among the cupriferous diorites on the St. Francis at Drummondville, and in the plumbaginous slates near the outlet of Lake Memphramagog.

This group of rocks appears in the province of Quebec along the southeast side of a great fault which has been traced from far below the City of Quebec to the line of Vermont on Missisquoi Bay. By this dislocation it is raised up along side of the rocks of the Trenton and Hudson river formations, and in one place is brought in contact with the Medina formation.

These higher strata, which are in some cases inverted along the line of fault, are often overlaid, and thus made to pass beneath the Quebec group. A little to the south of the province line this same fault brings up a still lower formation, the Red Sandrock of Vermont. This has been traced continuously from near Missisquoi bay to Shoreham, along the east side of the line of fault, while to the west appear the overturned and overlaid strata of the various formations from the Calciferous to the Hudson river formation inclusive. The line of fault is somewhat irregular, and where it has been affected by subsequent denudation, evidences of great overlapping are seen, the strata of the Hudson River formation, according to Sir William Logan, passing in some cases not less than a mile beneath the nearly horizontal layers of the Red Sandrock, which were by the earlier observers very naturally referred to the Medina formation, itself a red sandstone, and next in natural succession to the Hudson River formation in the New York series. In like manner the strata of the Quebec group, which, in Can-

ada, were found overlapping the Trenton limestone were regarded as belonging to the Hudson River formation until the study of their fossils led the way to a knowledge of their true age and relations.

The late Dr. Emmons supposed the Red Sandrock to include the Potsdam and Calciferous formations, though he at the same time completely misunderstood, as will be shown, its stratigraphical relations. Certain trilobites found in this formation by Prof. Adams in 1847 were recognized by Prof. Hall as belonging to the European genus *Conocephalus*, whose geological horizon was then undetermined, but which was afterwards shown to be a primordial type, and led Mr. Billings in 1861, to refer this sandrock to the Potsdam formation. (This Journal, II, xxxii, 232.) Subsequently the Rev. Zadock Thompson discovered in the slates of Georgia the trilobites which were by Emmons described as *Paradoxides*, but by Prof. Hall were recognized as a new genus *Olenellus*, of which he described two species, *O. Vermontana* and *O. Thompsoni*. Notwithstanding its designation, the Red Sandrock of Vermont consists in great part of a red or mottled granular dolomite associated with beds of fucoidal sandstone, conglomerate and slates.

This formation is well displayed in a section examined by Sir William Logan in Swanton, about two miles south of the boundary line between Quebec and Vermont, and described in the Geology of Canada, page 281. The thickness of the series here regarded as belonging to the Potsdam formation is 2,200 feet, intercalated in which, and about 500 feet from the base, occur 130 feet of gray and bluish-black slates, containing the two species of *Olenellus* noticed above, with *Conocephalites*, *Obolella*, *Orthisina*, &c. These slates have been traced southward to a locality in Georgia, where the same species of *Olenellus* occur in similar slates. The strata of the Potsdam formation in Swanton pass to the eastward beneath black shales and thin-bedded limestones holding the characteristic fossils of the Quebec group, and apparently representing the upper portion of the Levis formation. The superposition of these is well seen at Herrick's Mills, besides other places. The Levis formation may be traced northward around the extremity of the Potsdam rocks till it appears between these on the east, and the strata of the Trenton group on the west, forming a tongue which becomes narrower and disappears to the southward where the Potsdam comes directly up against the Trenton group. A little above this point, however, a section from Highgate Springs eastward takes us from the Trenton across a fault bringing up the Levis formation, followed at the distance of a mile farther eastward by a second dislocation by which the Potsdam is

brought up against the Levis, and again passes beneath it to the east, as in the Swanton section. By the second upthrow, a tongue of the Potsdam is carried northward a distance of about ten miles in the midst of the Quebec group. The details of this structure are fully given in the map and sections of this region by Sir William Logan, contained in the atlas accompanying the Geology of Canada. It is to be remarked that the lowermost beds of the Levis formation which are seen to the westward between the Trenton and the Potsdam, do not appear along the eastern limit of the latter, which is overlaid by beds belonging to the summit of the Levis, from which Sir William Logan conjectures a want of conformity between the two formations. The unconformability, in this vicinity, of the overlying rocks with the Potsdam is also to be inferred from the entire absence of the Calciferous from its place between the Potsdam and Levis formations in this locality, although in Newfoundland it appears in its proper position, and with a thickness of no less than 2,700 feet, resting upon more than 5,000 feet of rocks which are referred to the Potsdam group, and overlaid by the Quebec group, which there appears fully developed.

It happens then from the facts already set forth, that the Potsdam formation, which at its outcrop at the foot of the Adirondacks and Laurentides, includes only from 300 to 700 feet of sandstone, is represented a few miles to the eastward by not less than 2,000 feet of dolomites, sandstones and slates, and moreover that occupying a position between the Calciferous and Chazy formations, which are contiguous at their eastern outcrop, there becomes intercalated within this short distance, a fossiliferous group, several thousand feet in thickness. Sir William Logan explains these remarkable differences between the two regions in the following manner. At the beginning of the Silurian period, when the nucleus of the present continent was composed of Laurentian and Huronian rocks, partly rising into hills and in part forming a region of slight elevation, there were deposited in the surrounding seas the first fossiliferous beds of the period. To this succeeded a depression of the bed of the continent, upon the surface of which were then successively laid the shallow water deposits, which are known in New York as the Potsdam sandstone and the Calciferous Sandrock. Meanwhile strata characterized by a similar fauna to these, but having very different lithological character, were formed in the deeper waters which surrounded the continental plateau, this being submerged and elevated at intervals, became overlaid with beds which represent only in a partial and uninterrupted manner, the great succession of strata that were being

accumulated in the adjacent ocean, and which constitute to the eastward, the rocks of Quebec and Vermont, and in the west the great thickness of sediment contemporaneous with those which form the metalliferous series of Lake Superior.

Already, at this early period, movements were going on in the earth's crust, folding and dislocating the paleozoic sediments, within what we may call the oceanic area, while those of the continental plateau were at the same time left undisturbed. This Sir William Logan explains by the resistance offered by the solid continental gneiss. He says, "we may suppose that if a sufficient lateral pressure were applied to strata thus accumulated, and arranged, there would result a series of folds running in a direction at right angles to that of the force, with prevailing overturn dips toward the line of resistance. The solid crystalline gneiss offering more resistance than the newer strata, there resulted a break coinciding with the inclined plane at the junction of these with the gneiss. The lower paleozoic strata pushed up this slope would then raise and fracture the formations above, and be ultimately made to overlap the portion of those resting on the edge of the higher terrace, after probably thrusting over to an inverted dip, the broken edge of the upper formations. The shallow-water strata of the higher terrace relieved from the pressure by the break, would remain comparatively undisturbed, and thus the limit of the more corrugated area would correspond with the slope between the deep and shallow waters of the Potsdam period. The resistance offered by the buttress of gneiss would not only modify the main disturbance, but would probably also guide or modify in some degree, the whole series of parallel corrugations, and thus act as one of the causes giving a direction to the great Appalachian chain of mountains." (*Geol. of Canada*, p. 297.)

In further illustration of this view, I have to communicate a recent discovery which shows the progress of the change from the continental to the oceanic area, and demonstrates that at a point where the Potsdam had already assumed the character of the Red Sandrock, it was overlaid by all of the members of the New York series, up to the Hudson River group, inclusive. It is now some years since Mr. Billings of the Canada Geological Survey received from Messrs. C. H. Hitchcock and A. D. Hager, fossils apparently newer than the Quebec group, from a place to the east of the Potsdam in Sudbury, Vermont, and in 1866, the Rev. Augustus Wing, of Bridport in that state, correctly determined these to belong to the Trenton formation, and sent specimens of the fossils to Mr. Billings, who in April last, visited, and carefully examined the locality, with Mr. James Richardson. The following are the facts observed along a line

of section passing from Crown Point in New York across the head of Lake Champlain and through Bridport to Cornwall in Vermont. Passing from the Laurentian across the various members of the New York series, we reach the limestones of the Trenton group of the east shore of the lake and then encounter the Chazy formation which is brought up along a gentle anticlinal, and is succeeded by the Trenton, Utica, and Hudson River formation all sloping gently to the eastward until we come to the great fault which brings up the Red Sandrock. All the preceding details are shown on Mr. Hitchcock's map which accompanies the Geology of Vermont. This red sandrock which is here seen to overlap the Hudson River formation dips eastward at a small angle and is overlaid by a great mass of limestone holding the characteristic fossils of the Calciferous, among which, as determined by Mr. Billings, are *Ophileta complanata*, *Bathyurus conicus*, *Asaphus canalis*, and others too obscure to be identified. These fossils were first discovered by the Rev. Mr. Wing in Sudbury. At the summit of the formation, Mr. Billings was enabled to detect other obscure forms which resemble those of the Levis formation, and seem to indicate the first appearance of the Quebec group in its place between the Calciferous and Chazy formations. Next in ascending sequence, occurs a mass of limestones not less than 2,000 feet in thickness, and probably representing both the Chazy and the Trenton. From it the following Trenton species were obtained by Mr. Billings in Sudbury: *Stenopora fibrosa*, *Petraia corniculum*, *Glyptocrinus ramulosus*, *Ptilodictya acuta*, *Leptæna sericea*, *Strophomena alternata*, *Orthis testudinaria*, *O. pectinella*, *Pleurotomaria lenticularis*, *P. rotuloides* and *Trinucleus concentricus*.

To the east these limestones are cut off by a second dislocation, which brings up the Levis formation, with its characteristic fossils against the Trenton. A ravine marks the line of fault, which has been followed for a distance of more than twenty miles from the southern line of Sudbury northward to Weybridge, where it joins the great western fault. To the west of this line, fossils of the various divisions of the Trenton group are found in numerous localities, and to the east, the characteristic forms of the Levis formation, among which Mr. Billings has detected *Pleurotomaria Quebecensis*, *P. Missisquoi*, *Murchisonia Vesta*, *Ophileta bella*, *Maclurea matutina*, *M. pondersa*, and *Bathyurus Saffordi*, besides which many large Orthoceratites are seen in section. These fossils are found in many localities in Sudbury, Cornwall, Middlebury, and Brookville. The limestones are for the most part bluish, but are closely associated with the white marbles quarried in these

localities. Thus at Brookville, Mr. Billings found Levis fossils immediately above and below the white marbles, clearly showing these beds of white crystalline limestone to belong to the lower portion of the Quebec group. This confirms the view expressed by me in this Journal for May, 1861, page 392, that these marbles represent a great development of calcareous matter in the Quebec group and are to be regarded as beds of chemically precipitated carbonate of lime, and not limestones of organic origin. Their great purity and crystalline texture depend upon original conditions of deposition, and not upon any subsequent alteration.

In this connection it may not be out of place to notice some of the views of the late Dr. Emmons which have lately been resuscitated, with certain modifications, by the Rev. J. B. Perry, in a communication to the Boston Society of Natural History in December last. Dr. Emmons conceived the Red Sandrock to be the base of the Silurian system, and to rest upon an older and unconformable series of rocks, to which he gave the name of the Taconic system. His views, though rejected by all American geologists, have been supported by Mr. Jules Marcou, who, however, differs from Dr. Emmons in including the Red Sandrock in the Taconic system, in which he is followed by Mr. Perry. For an exposition of the views of Dr. Emmons and of Mr. Marcou's curious misconception of them, the reader is referred to this Journal, II, xxxii, 427, and xxxiii, 135, 281.

According to Mr. Perry, the slaty rocks which appear to the east and west of the Red Sandrock, belong to two older formations or divisions, which he describes as Lower and Middle Taconic. In the latter he includes the Georgia slate with its primordial species and the Swanton slate considered to be still lower, with graptolites and *Atops trilineatus* Emmons, while he refers to the Lower Taconic, the slates, limestones and conglomerates of the Quebec group. This Middle Taconic is made up of three distinct series of black and brown shales, none, however, older than the Potsdam:

1st. His Swanton slates. These are the Utica and Hudson River formations which are distinctly overlapped by the Potsdam at various points along its western border, as at Sharpshins and Appletree Point, near Burlington, where the superpostion is clearly seen, and where *Triarthrus Beckii* (*Atops*), is found in the underlying Hudson River slates. An excellent section, showing the superposition of the Potsdam to the Hudson River formation at St. Albans, is given by Mr. Hitchcock in the Geology of Vermont, page 374.

2d. The Georgia slates which are but an interstratified part of the Potsdam, as appears from the section at Swanton. The

Conocephalites Teucer Billings, cited by Mr. Perry as characteristic of these supposed lower slates, is, according to Mr. B., common to the slates and to the associated Red Sandrock.

3d. The black slates to the east of the Potsdam which belong to the Levis formation, and afford its characteristic fossils at various points all along their outcrop. We have seen that the Levis formation with its graptolitic slates had been at an early date confounded with the Utica and Hudson River formations in Canada, and all of these have now been confounded with the Georgia slates to make up the Middle Taconic. Finally, it remains to be said that this view consigns to a still more remote period, the lower Taconic, the great mass of the Quebec group which lies between the lines of fault already alluded to and the Green mountains, including the white marbles or Eolian limestones with their associated fossils, the Orthoceratites, Maclureas and other fossils already named, which no paleontologist would think of assigning to a horizon below the Potsdam.

To sum up in a few words—all the evidence, paleontological and stratigraphical, as yet brought forward, affords no proof of the existence in Vermont of any strata (a small spur of Laurentian excepted) lower than the Potsdam formation, which the present advocates of the Taconic system regard as forming its summit. The supposed more ancient Middle and Lower Taconic clearly consists in part of Potsdam, in part of Utica and Hudson River, and in part of the Quebec group, which also constitutes the Lower Taconic. To the upper portion of the Quebec group, the Geological Survey of Canada have already referred the gneiss of the Green mountains, assigning to this chain a synclinal structure, nor does there yet seem to be any reason to believe otherwise.

That strata still older than the Potsdam of New York and Vermont were deposited in some portions of the oceanic area, is apparent from the existence in New Brunswick of the St. John's slates holding a primordial fauna older than the Potsdam, and it is not impossible that their equivalents may underlie the Potsdam formation of Vermont. No such rocks have, however, as yet, been detected either in Vermont or Canada, and to preserve the name of the Taconic system as the designation of a series of rocks older than the Potsdam and lying unconformably beneath it, is simply to perpetuate an unfortunate mistake which I believe Dr. Emmons, if now living, would, with the paleontological evidence at present before the world, be the first to acknowledge.

ART. XXI.—*Contributions from the Sheffield Laboratory of Yale College.* No. XVII.—*On Willemite and Tephroite;* by WILLIAM G. MIXTER, Ph.B., Assistant in the Sheffield Laboratory.

THE willemite occurring in the well known franklinite and zincite veins at Mine Hill and Stirling Hill in Sussex Co., New Jersey, has been frequently analyzed; but the results obtained by different chemists vary considerably.* This variation may be explained in part by the fact that specimens taken from near the surface are sometimes altered by the oxydation of the manganese. This is especially true of many specimens of the so-called troostite which on the exterior of the crystals are blackened and quite opaque, while the interior is often a translucent to transparent honey-yellow mineral. There is furthermore a great variation in color even of the unaltered mineral, which is found shading from almost pure white to apple-green through different tints of yellow and red.

Having recently visited the locality and obtained some remarkably fresh and unaltered specimens, I have undertaken their analyses with the following results on three different varieties.

I. *Apple-green Willemite.*—This variety is found in considerable abundance on Mine Hill, but is apparently of not so frequent occurrence at the Stirling Hill mines. It has a bright apple-green color, shading in some instances almost into white. In most specimens it is intimately mixed with franklinite, zincite and calcite, but the masses are often sufficiently large to allow of its easy selection free from these minerals. No difficulty was experienced in obtaining the green mineral perfectly pure for analysis. Specific gravity =4·16 (at 17° C.) Hardness =5·5. A qualitative analysis indicated besides zinc and silica, a considerable portion of manganese and traces of iron and magnesia. The mineral readily gelatinizes with acids and after the separation of the silica in the usual manner, the quantitative separation of the iron was effected by precipitating it as basic-acetate, and from the filtrate the manganese was thrown down as oxyd by oxydation with bromine, this oxyd was filtered off, washed and re-dissolved and precipitated with carbonate of soda and weighed as proto-sesquioxyd. The zinc was thrown down from the filtrate from the oxyd of manganese as sulphid, subsequently re-dissolved and precipitated by carbonate of soda. The magnesia was separated from the filtrate from the sulphid

* For analyses, see Dana's Mineralogy, 5th ed., p. 262.

of zinc as ammonio-phosphate and weighed as pyro-phosphate. Great care was taken to ensure the complete separation of the different bases and the purity of the different precipitates. Two analyses gave:—

	1.	2.	Mean.	Oxygen.	
Silica,	27·43	27·36	27·40	14·61	14·61
Zinc-oxyd,	66·83	----	66·83	13·19	
Manganous oxyd,	5·69	5·78	5·73	1·29	14·49
Ferrous oxyd,	0·06	0·06	0·06	·01	
Magnesia,	trace	trace	trace		
Ignition,	0·18	0·18	0·18		
	100·19		100·20		

II. *Honey-yellow Willemite.*—This variety occurs in hexagonal crystals with rhombohedral terminations imbedded in the highly crystalline and cleavable limestone as well as sometimes intimately associated with the zincite and franklinite. The specimen selected for analysis was translucent, almost transparent, of a honey-yellow color, and formed the interior of a crystal an inch or more in diameter, which exteriorly was of a pale yellowish to flesh-red color. The mineral was entirely free from any traces of franklinite or zincite. Specific gravity =4·11. Hardness=5·5. B.B. same as No. 1. Analyses conducted as before described, gave

	1.	2.	Mean.	Oxygen.	
Silica,	27·75	28·09	27·92	14·89	14·89
Zinc oxyd,	58·05	57·62	57·83	11·42	
Manganous oxyd,	12·62	12·57	12·59	2·84	14·86
Ferrous oxyd,	0·49	0·74	0·62	·14	
Magnesia,	1·15	1·12	1·14	·46	
Ignition,	0·28	0·28	0·28		
	100·34	100·42	100·38		

III. *Ash-gray mineral.*—This variety is found at the north opening on Mine Hill. Some specimens show a distinct cleavage in one direction, while on others, little or no cleavage can be observed; and although the mineral was at first suspected to be tephroite, still the decision that it was this species was not fully reached until it was analyzed. It occurs in large mases, but contains disseminated through it, minute spangles of zincite, sometimes giving it a reddish tinge and rendering it exceedingly difficult to select pure material for analysis. Specific gravity=4.* Hardness =5·5. B.B. the mineral is more fusible than green and honey-yellow willemite, and instead of yielding

* By a typographical error, the specific gravity of the magnesian tephroites examined by Collier and Hague is stated in my article (this Journal, II, xxvii, 66), to be 2·97 and 2·87, instead of 3·97 and 3·87.—G. J. B.

a white enamel, turns black and gives a black glass on fusion. Two analyses of the mineral selected with great care to free it from the zincite yielded:

	1.	2.	Mean.	Oxygen.	
Silica, ...	29·42	29·46	29·44	15·70	15·70
Zinc oxyd, ...	7·36	7·36	7·36	1·45	
Manganous oxyd,	57·55	57·07	57·31	12·91	
Ferrous oxyd, ...	0·89	0·84	0·87	·19	16·27
Lime, ...	2·51	2·52	2·51	0·72	
Magnesia, ...	2·49	2·51	2·50	1·00	
Ignition, ...	0·27	0·27	0·27		
	100·49	100·03	100·26		

The method employed in the analyses was essentially the same as in the other minerals, except that the lime was separated from the filtrate from the sulphid of zinc as oxalate prior to the precipitation of the magnesia as phosphate.

The results of these analyses prove the three minerals to be all true unisilicates. In I, we have the oxygen ratios of bases to silica as 14·49 : 14·61, in II, 14·86 : 14·89. The green willemite contains 5·77 per cent of manganous oxyd while the very pure honey-yellow mineral contains 12·59 per cent, thus differing in composition very materially from the willemite of European localities. This is a much higher percentage of manganous oxyd than obtained in any other analysis of the American mineral. The ash-colored mineral, as appears from the analysis, is proven to be tephroite. It bears a close resemblance to a specimen of the original tephroite received from Prof. Breithaupt by Prof. Brush, but it was first thought that it differed from this chemically in having a portion of the manganese replaced by zinc. A more careful examination of the specimen analyzed with a magnifier, shows that besides having zincite as a mechanical impurity, it also contains dissemiated through it minute specks of pale green, almost white, willemite, and it was not found practicable to select the tephroite for analysis free from this impurity. But specimens since found at the locality appear to be entirely free from this admixture. The analyses are of interest as identifying the *ash-gray* variety of tephroite originally described by Breithaupt, and which hitherto had not been re-discovered at the locality. It occurs in considerable abundance at the north end of Mine Hill, and may be readily distinguished from willemite by its ash-gray color, its pyrognostic characters, and its ordinary feldspar-like cleavage.

New Haven, June 25th, 1868.

Art. XXII.—*Notices of papers in Physiological Chemistry.* No. I.—By George F. Barker, M.D.

1. *On Hæmatoidin.*

Holm has published* the results of a research upon this substance, made in the laboratory of Professor Städeler in Zurich. The investigation was undertaken for the purpose of fixing more accurately the properties of hæmatoidin, as well as to settle the question of its identity with bilirubin. To this end, Holm first prepared this latter body, both from gall-stones and from human bile; confirming minutely all the properties previously assigned to it by Städeler.

The hæmatoidin was obtained from several sources. Apoplectic clots in the brain were first used; but the quantity thus obtained was small, permitting merely the fact of the yellow color of the chloroform solution and its change to green on exposure to light, to be noticed. The yellow masses from the ovaries of the cow were taken for a final trial. These corpora lutei contain hæmatoidin crystals, many of those figured by Funke being from this source. When an ovary is divided lengthwise, the most recent yellow bodies, which appear upon the exterior of the organ as large yellowish-red conical protuberances, are found to consist of a soft juicy mass, colored various shades of yellow, and crossed by radiating lines. The older bodies are cinnabar-red in color, dry, and granular. Well-formed crystals were rarely observed even with a magnifying power of 300 diameters; though aggregations of short needles, and irregular reddish tables were noticed, which, especially in the younger bodies, were mixed with yellow fat. Nitric acid containing nitric peroxyd tinged the mass greenish-blue, transiently.

Both chloroform and carbonic disulphid may be used for the extraction of hæmatoidin, since both yield it crystallized. Holm's process of preparation was as follows: All the red and yellow bodies were cut from the ovary with scissors, freed as completely as possible from the surrounding tissues, and rubbed to a fine pulp with pounded glass; this pulp was then placed in a flask with chloroform, and allowed to stand for several days, being frequently agitated. Upon filtration, a deep gold-yellow solution was obtained, which on spontaneous evaporation, left a semi-fluid yellowish-red fatty mass. The fat was evidently a solvent of the hæmatoidin, since it was not until after some days that crystallization began. As it proceeded, the fat gave

* J. pr. Ch, c, 142, March, 1867.

up its coloring matter to the crystal; leaving finally single and nearly colorless fat drops, each of which contained a small, but very beautifully defined hæmatoidin crystal.

If the formation of these crystals be watched under the microscope with a power of 150 diameters, they will first be seen as acute three-sided tables, one of the sides being frequently curved. Generally, two of these triangular crystals unite by their bases, thus giving the rhombic table, so characteristic of hæmatoidin. Sometimes there is an indentation at the obtuse angle of the rhomb, into which other pairs of twins may grow, producing four pointed stars. And these, by the filling in of their reëntering angles, pass into square tables, which increase in thickness and resemble cubes. The obtuse angle of the rhomb too, is often rounded.

Hæmatoidin is one of the most beautiful substances known to organic chemistry, resembling murexid very closely. By reflected light, perfect crystals show a magnificent cantharides-green color and a metallic luster; by transmitted light they appear red. If not too thick, the crystals, as viewed under the microscope, seem to be a pure magenta red; and a number of crystals irregularly aggregated together show a rich blue or violet tint.

The complete purification of these crystals from the adhering cholesterin and fat, is exceedingly difficult. After removing the greater part of the fluid fat by agitation with absolute alcohol, a tenacious fatty mass was left which dissolved only in ether. As the hæmatoidin is also soluble in this menstruum, a great loss was experienced in this process, the quantity obtained being insufficient even for an elementary analysis. A second portion not quite free from fat, was recovered from the etherial solution. This was used for the detection of iron, by incinerating it on platinum foil. A gray speck remained, which on being dissolved in chlorhydric acid, gave a slight greenish tint on the addition of potassic ferrocyanid. On account of the small quantity of substance used, and also of its not being perfectly pure, Holm is unwilling to decide upon the presence of iron in hæmatoidin.

After treatment with ether, the crystals have no longer the brilliant cantharides-green luster, their surfaces being etched by this solvent. They now resemble freshly prepared chromic acid. Moistened while in the field of the microscope, with nitric acid containing nitric peroxyd, a bright but transient blue color is developed.

Hæmatoidin is very readily soluble in chloroform, giving a gold-yellow solution; and in carbonic disulphid with a flame-red color, or orange if greatly diluted. Less soluble in absolute

ether, not at all in absolute alcohol and water, nor in ammonia, sodic hydrate, dilute mineral acids (non-oxydizing) or dilute acetic acid.

From a close comparison of hæmatoidin with bilirubin, Holm concludes :—That these bodies have not only different forms and colors, but also essentially different chemical properties. While bilirubin possesses the properties of a weak acid and combines with bases, hæmatoidin is quite an indifferent body. Bilirubin dissolves in carbonic disulphid with a gold-yellow, hæmatoidin with a flame-red color (orange, if greatly diluted). Bilirubin is insoluble in ether, hæmatoidin is easily soluble. If a bilirubin solution in chloroform be shaken with ammonia, the bilirubin is completely removed from the chloroform, which becomes colorless, while the alkaline solution becomes yellow; hæmatoidin on the other hand, is not removed from chloroform by alkaline solutions. Indeed these bodies may thus be perfectly separated. Bilirubin shows in solutions containing alcohol, even when only the smallest traces are present, upon adding nitric acid containing nitric peroxyd, a magnificent play of colors, green, blue, violet, red and yellow, while a similar solution of hæmatoidin is simply decolorized.

2. *On the coloring matter of the yolk of eggs.*

STÄDELER[*] has made some experiments with the coloring matter of the yolk of the egg, in order to make a comparison between it and the bile pigment, to which it had been compared by Chevreul.

If the fresh yolk be agitated with ether, the coloring matter and the fat are dissolved, and the solution leaves upon evaporation a yellow fatty mass, which gives a dirty bluish-green color when mixed with nitric acid containing nitric peroxyd, or with a few drops of concentrated sulphuric acid. If this egg-oil be heated with a five per cent solution of sodic hydrate to saponify the fat, an unpleasant odor like that of fish livers, is developed; and upon agitating the saponified mass with ether, it is deprived of all its coloring matter. This behavior proves the absence of bile-pigment, since the compound of bilirubin with sodium, is insoluble in ether. By evaporation, the etherial extract leaves a deep gold-yellow difficultly saponifiable fat, which by the crystallization of the cholesterin, solidifies to a butter-like mass. This fat, rich in pigment, possesses great similarity with the fat containing hæmatoidin, obtained from the ovaries. Nitric acid, when colored, develops a pure blue tint, which is transient. Ether and chloroform dissolve it to a gold-yellow solution; this, mixed with alcohol and treated with colored nitric acid, is simply decolorized, affording no play of

[*] J. pr. Ch., c, 148, March, 1867.

colors. With carbonic disulphid an orange-red solution is obtained. And finally, alkalies do not remove the pigment from its solution in chloroform.

From these results, Städeler concludes that the coloring matter of the yolk of the egg is either hæmatoidin itself, or a body very similar to it in properties.

3. *On the chemical constituents of the Supra-renal capsules.*

Holm has also published* the results of a new examination of the supra-renal glands. These organs were collected fresh from the slaughter-house, and, when freed carefully from adhering fat, weighed 650 grams. They were rubbed to a fine pulp with glass-powder, mixed with twice their volume of strong alcohol, digested at a gentle heat, pressed out and filtered, the alcohol distilled off, and the remaining fluid precipitated with neutral plumbic acetate. This lead precipitate was not farther examined. The color of the filtered fluid was brownish-yellow, and the edge of the filter in contact with the air acquired a violet tint. The fluid was now precipitated with basic plumbic acetate, and allowed to stand 12 hours. The precipitate was then collected, and the filtrate treated with cupric acetate and heated to boiling, by which a second precipitate was formed, mixed with a considerable quantity of red cuprous oxyd. This copper precipitate being separated by filtration, left a purple colored fluid.

In order to extract completely the uric acid and the xanthin existing in the tissue, the pulpy mass left from the above treatment with alcohol, was digested with water at 50° C.; and the liquid thus obtained was treated successively with neutral and with basic plumbic acetate, and with cupric acetate. The basic lead precipitate and the copper precipitate were united with the similar precipitates obtained in the first process, and examined together.

(1.) *Basic lead precipitate.*—This contained no uric acid but afforded a considerable quantity of inosite, which was easily recognized by its sweet taste, easy solubility, non-precipitability by ammonic carbonate, crystalline form, and behavior with nitric acid, calcic chlorid, and ammonia.

(2.) *Copper precipitate.*—This was free from xanthin, but contained considerable hypoxanthin, easily soluble in dilute chlorhydric acid, and yielding on evaporation a salt crystallizing in needles, which, when decolorized with animal charcoal, evaporated to dryness with ammonia, and extracted with water, left pale yellow hypoxanthin. This, when dissolved in dilute nitric acid and carefully evaporated, left a scarcely yellow spot, be-

* J. pr. Ch., c, 150, March, 1867.

coming citron-yellow on stronger heating, and giving a splendid purple color on moistening with sodic hydrate.

(3.) *Filtrate.*—As already mentioned, the filtrate had a dingy purple tint. It was treated with sulphydric acid, the deposited sulphids were removed and it was then evaporated on the water-bath. During this process, the pigment separated as a violet pellicle. The fluid filtered from this, afforded by farther treatment, taurin; and beside this, a few small rounded masses were detected by microscopic examination, which may have been leucin, since Neukomm states that these glands, observed during Bright's disease afforded him this substance. On the other hand, however, Seligsohn affirms that the supra-renal capsules contain no leucin, while Virchow asserts that they contain it in large amount.

The violet pigment which separated during the evaporation of the filtrate, was found to be insoluble in alcohol, ether, chloroform, carbonic disulphid, and benzol. Alkalies took up only a trace of it, perhaps only an impurity, the filtrate being yellow, and the undissolved pigment remaining unchanged in color. Water was found to dissolve it readily when acidulated with any mineral acid; it was also dissolved by moderately dilute acetic acid, on boiling. These acid solutions were yellow; and from them the entire coloring matter separated again in violet flocks or the addition of ammonia. This pigment appears therefore, to be a weak base. Unfortunately, the quantity obtained was too small to allow of a more thorough examination; a fact which Holm regrets, inasmuch as this coloring matter, as he suggests, appears to be the pigment which produces the peculiar bronzing of the skin, which is characteristic of Addison's disease.

Arnold has stated that the coloring matter of the supra-renal capsules is precipitable by plumbic acetate. Holm maintains that there is but one chromogen in these glands, which is not precipitable by the lead salt, and which passes into the pigment by oxydation. He supports this view by the fact that the yellow alcoholic extract, which becomes red in contact with light and air, affords, after precipitation with plumbic acetate, in consequence of the farther absorption of oxygen, a violet color, even in the presence of free acids. The complete conversion, however, of the chromogen into pigment takes place only when the solution is boiled with cupric acetate; the cupric oxyd giving up a part of its oxygen and becoming cuprous oxyd.

4. *On the rational formula of Urea.*

Heintz concludes an exceedingly interesting paper upon the compounds of trioxethylenamic (triglycolamic) acid,* by com-

* Ann. Ch. Pharm., cxl, 264, Dec., 1866.

paring the constitution of its amids with that of urea. Having shown that the formula of ethylic trioxethylenamate* (triglycolamic ether) is $((\text{Є}_2\mathrm{H}_2\Theta)\Theta\mathrm{Et})'_3\mathrm{N}$, it follows that trioxethylenammonamid (triglycolammonamid) prepared from it by the action of ammonia, is simply $((\text{Є}_2\mathrm{H}_2\Theta)\mathrm{H}_2\mathrm{N})'_3\mathrm{N}$; being ammonia in which the three atoms of hydrogen are replaced by oxethylenammonium, $((\text{Є}_2\mathrm{H}_2\Theta)\mathrm{H}_2\mathrm{N})'$, a monad radical. This amid is a monacid base; though it contains four nitrogen atoms which are not directly united to oxygen, but are saturated only by bonds of hydrogen or other radicals; from which we might expect it to be tetracid.

It is a well known fact, however, that when the hydrogen in ammonia or ammonia-like bodies, is replaced by negative or oxygenated radicals, the resulting compounds are either not at all basic, or are only feebly so; indeed, this fact characterizes the class of substances termed amids. Trioxethylenamic (triglycolamic) acid for example, can unite together no more acid molecules when converted into an amid than it could before. But, while the compounds formed by the acid itself with other acids, are very feeble, none of them existing in the solid form; when the hydryl atoms of the acid are replaced by amidogen atoms, thus forming the amid, the basic power is raised in intensity and the new compound yields well crystallized substances with acids. While therefore, no more acid molecules can be held after the introduction of nitrogen atoms than before, yet the same number are now held more firmly.

There is a large class of bodies which contain more nitrogen atoms than is equal to the number of monobasic acid molecules with which the substance can unite. These bodies are called ureas; and, though they contain two nitrogen atoms in the molecule, they are all monacid. They all contain, too, an oxygenated radical. The composition of these bodies may be easily brought into comparison with that of trioxethylenammonamid (triglycolammonamid); and in the similarity there observed, Heintz finds the true explanation of the constitution of the ureas.

* $(\text{Є}_2\mathrm{H}_4)$ being the positive bivalent radical ethylene, the negative radical corresponding to it is $(\text{Є}_2\mathrm{H}_2\Theta)''$, oxethylene (formerly glycolyl); whence oxethylenic (glycollic) acid is $(\text{Є}_2\mathrm{H}_2\Theta)\left\{\begin{matrix}\Theta\mathrm{H}\\ \Theta\mathrm{H}\end{matrix}\right.$, oxethylenamic (glycolamic) acid is $(\text{Є}_2\mathrm{H}_2\Theta)\left\{\begin{matrix}\Theta\mathrm{H}\\ \mathrm{NH}_2\end{matrix}\right.$, and tri-oxethylenamic (tri-glycolamic) acid is

$$\mathrm{N}\left\{\begin{matrix}((\text{Є}_2\mathrm{H}_2\Theta)\Theta\mathrm{H})'\\ ((\text{Є}_2\mathrm{H}_2\Theta)\Theta\mathrm{H})'\\ ((\text{Є}_2\mathrm{H}_2\Theta)\Theta\mathrm{H})'\end{matrix}\right.$$

In the first place, then, the formula should indicate the monacid character of urea; hence, it may be written $N\left\{\begin{matrix}(\mathrm{CO})''\\(\mathrm{NH_4})'\end{matrix}\right.$, a formula recommended by its simplicity; or $N\left\{\begin{matrix}((\mathrm{CO})\mathrm{H_2N})'\\ \mathrm{H}\\ \mathrm{H}\end{matrix}\right.$ In both cases, ammonium radicals replace the hydrogen of ammonia. Which of these formulas is the more correct?

The ureas are distinguished: 1st, by their composition; being ammonia, in which carbonyl (CO) a bivalent negative radical, has replaced hydrogen; and 2nd, by their monacid character. Under this definition are included the compound ureas also, formed from ordinary urea precisely as ethylamine, etc., are formed from ammonia. Heintz proposes to extend this definition, and to call any ammonia-like body which contains a bivalent acid radical and which is monacid, a urea. Such a compound for example, containing not carbonyl, it is true, but the bivalent acid radical oxethylene ($\mathrm{C_2H_2O}$), would be formed by treating ethylic oxethylenamate (glycolamate), with ammonia. It would be monacid, and, if urea be written $N\left\{\begin{matrix}(\mathrm{CO})''\\(\mathrm{NH_4})'\end{matrix}\right.$, would be $N\left\{\begin{matrix}(\mathrm{C_2H_2O})''\\(\mathrm{NH_4})'\end{matrix}\right.$*. It stands in the same relation to trioxethylenammonamid which oxethylenamic acid (glycolamic acid or glycocoll) does to trioxethylenamic (triglycolamic) acid. But it has already been proved above, that in this tertiary amid, the negative radical replaces hydrogen which is contained within the ammonium group; and hence that its formula is $N\left\{\begin{matrix}((\mathrm{C_2H_2O})\mathrm{H_2N})'\\((\mathrm{C_2H_2O})\mathrm{H_2N})'\\((\mathrm{C_2H_2O})\mathrm{H_2N})'\end{matrix}\right.$. Of course then, the formula of the primary amid is $N\left\{\begin{matrix}((\mathrm{C_2H_2O})\mathrm{H_2N})'\\ \mathrm{H}\\ \mathrm{H}\end{matrix}\right.$, and its name is oxethylenammonamid. Since therefore, urea is analogous to this primary amid, both in composition and formation, (being produced by the action of ammonia upon urethane (ethylic carbamate)), it follows that the formula of urea is $N\left\{\begin{matrix}((\mathrm{CO})\mathrm{H_2N})'\\ \mathrm{H}\\ \mathrm{H}\end{matrix}\right.$; and that this body is carbammonamid, the amid of carbamic acid, precisely as oxethylenammonamid is the amid of oxethylenamic acid (glycocoll).

* This substance has not been actually prepared; though Heintz promises experimental proof of the accuracy of the above reaction.

ART. XXIII.—*Contributions from the Sheffield Laboratory of Yale College.* No. XVIII.—*On Sussexite,* a new borate from Mine Hill, Franklin Furnace, Sussex Co., New Jersey;* by GEORGE J. BRUSH.

IN examining a specimen of a fibrous mineral, obtained at Mine Hill last year, I found that it was a fibrous silicate of zinc, and being desirous of further investigating the mineral, I requested my assistant, Mr. Wm. G. Mixter, on his recent visit to the locality, to obtain as much of the fibrous substance as possible, so that a quantitative analysis might be made of it. Mr. Mixter was fortunate in obtaining one specimen of what we at first sight took to be the fibrous silicate, but on examination of its pyrognostic characters it proved to be a new mineral, a hydrous fusible borate, reacting strongly with the fluxes for manganese. This interesting discovery led me at once to revisit the locality, and I there succeeded in obtaining enough of the new mineral to give the following characters. It is found in the franklinite vein at the opening on the north end of Mine Hill, associated with franklinite, zincite, willemite, tephroite, calcite and what appears to be a double carbonate of manganese and magnesia occurring implanted on, or imbedded in, the fibrous mineral in minute hemispherical forms; it has also associated with it a black hydrate of manganese, apparently the species manganite, and a pale pink carbonate which is probably rhodochrosite. The black manganite and the double carbonate have the appearance of being products of the alteration of the borate, since where associated with these the latter seems exceedingly friable and evidently in process of decomposition.

The pure mineral is whitish with a tinge of yellow or pink, is translucent on the edges and in thin fragments, and possesses a silky to pearly luster. The structure is fibrous, sometimes asbestiform, although in other specimens it seems to cleave much more readily in one direction than in a direction at right angles to this, yielding flat fibrous fragments. The mineral occurs in seams in calcite, sometimes with the fibers running transversely, and in other specimens quite long and parallel to the seam. The hardness is slightly above 3, scratching calcite, but not aragonite. Specific gravity=3·42.

On heating in the closed tube the mineral darkens slightly in color and yields water which reacts neutral to test-papers; but if turmeric paper is moistened with this water, then with

* The discovery of this mineral was announced in the July number of this Journal, p. 140.

a drop of dilute chlorhydric acid and afterwards dried, it assumes the red color, characteristic of boric acid, and thus shows that at least a trace of this acid is driven off with the water. In the forceps the mineral fuses in the flame of a candle (F.=2) and B.B. in O. F. yields a black crystalline mass and colors the flame intensely yellowish-green. With borax and salt of phosphorus gives a deep amethystine bead in O. F., which in R. F., becomes colorless and transparent. With soda yields a green manganate.

It is readily dissolved in chlorhydric acid, and most specimens thus treated give off a minute quantity of chlorine, showing traces of a slight alteration of the protoxyd of manganese into a higher oxyd. On evaporation to dryness and resolution in acid, minute imponderable traces of silica were found. Qualitative analysis proved the presence of boric acid, manganese, magnesia, and water, with questionable traces of zinc and soda. A fragment of the mineral moistened with sulphuric acid and held in the flame of an ordinary Bunsen burner gave, when observed through the spectroscope, the characteristic spectrum of boric acid.

The exceedingly simple composition of the mineral rendered the quantitative determination of the bases comparatively easy. The mineral being dissolved in chlorhydric acid, the excess of acid was driven off and the manganese was thrown down by bromine in the presence of an excess of acetate of soda as hydrated sesquioxyd; this was re-dissolved and precipitated as ammonio-phosphate and weighed as pyro-phosphate.* The magnesia was separated from the filtrate from the oxyd of manganese, (after it was first ascertained that this solution was entirely free from manganese) as ammonio-phosphate and estimated as pyro-phosphate. The water was determined by igniting the powdered mineral in a glass tube closed at one end, about 10 inches in length with a calibre of ¼ of an inch. The length of the tube effected a complete condensation of the water, which was deposited on the interior five or six inches from the open end, and the tube and contents on being weighed proved to have suffered a loss of less than one milligram by the ignition. The water was then dried out at the ordinary temperature in vacuo over chlorid of calcium. To make entirely sure that no boric acid went off with the water, I ignited a portion of the mineral which had previously been thoroughly mixed with about five times its weight of calcined magnesia and then covered with a layer of pure magnesia. The results of this experiment

* For details of this admirable method for the estimation of manganese see T. S. Henry, Phil. Mag., IV, xvi, 197; and W. Gibbs, this Journal, II, xliv, 216.

confirmed the water determination made by the above method. The boric acid was determined by Stromeyer's method as borofluorid of potassium. The results of the analyses are:—

	1.	2.	3.	4.	5.	6.	Mean.	Oxygen.
Boric acid,	----	----	----	---	---	31·89	31·89	22·82
Manganous oxyd,	40·08	40·20	40·01	---	---	---	40·10	9·04
Magnesia,	17·12	16·76	17·21	---	---	---	17·03	6·81
Water,	----	----	----	9·64	9·53	---	9·59	8·53
							98·61	

The analysis shows a loss of 1·39 pr. ct. doubtless due chiefly to the imperfections of the method employed for determining the boric acid. Calculating the loss as boric acid, the total amount of the acid is 33·28 pr. ct. and the oxygen ratio for $\dddot{B}$, $\dot{R}$ and $\dot{H}$ is 22·82 : 15·85 : 8·53 or 3 : 2·08 : 1·12. The ratio 3 : 2 : 1, although not according precisely with the analyses, is nevertheless probably the true ratio. It requires a change of but a few tenths of a per cent of water to make this ratio. In fact, in what appeared to be a fresher and less altered specimen than that above analyzed, I obtained but 8·93 pr. ct. of water, which would change the amount of boric acid calculated as loss to 33·94 pr. ct. Correcting the oxygen to correspond to these, we have for $\dddot{B}$, $\dot{R}$, $\dot{H}$, 23·27 : 15·85 : 7·94, or almost exactly 3 : 2 : 1, or considering the water basic, a ratio of 1 : 1, thus bringing out a most interesting relation between this species and native boric acid which has the formula $\dot{H}^3\dddot{B}$. Sussexite may be regarded an analogous compound in which $\frac{2}{3}$ of the water is replaced by manganese and magnesia, and we may write for its formula $(\frac{2}{3}(\dot{M}n, \dot{M}g)+\frac{1}{3}\dot{H})^3\dddot{B}$, or if the water be not considered basic it may be represented by $(\dot{M}n, \dot{M}g)^2\dddot{B}+\dot{H}$. The former I believe to be the correct view of the composition of the mineral.

In some of its physical and chemical characters sussexite resembles the mineral Szaibelyite from southern Hungary. This mineral is found imbedded in limestone in needle-like crystals, has a hardness of over 3, a density of 3, and is a hydrous borate of magnesia. One variety analyzed by Stromeyer gave the oxygen ratio of $\dddot{B}$, $\dot{M}g$, $\dot{H}$, 17 : 14·1 : 4, or of acid to bases including water of 17 : 18·1, or nearly 1 : 1, requiring but a slight change in the determination of water to make this also a mineral analogous in composition to boric acid, with which indeed it is already classified by Prof. Dana in the recent edition of his Mineralogy.* Another member of the group is Hydroboracite, a hydrous borate of magnesia and lime with the oxygen ratio of $\dddot{B}$, $\dot{R}$, $\dot{H}$, 4 : 3 : 1 or $(\frac{1}{2}(\dot{C}a, \dot{M}g)+\frac{1}{2}\dot{H})^3\dddot{B}$. Sussexite is at present a rare mineral, but as it occurs in a vein which is extensively

* Dana's Mineralogy, 5th ed., p. 593.

mined, there is every reason to hope that it may become more abundant. Its pyrognostic properties are so very characteristic that it may readily be distinguished from any other mineral which it resembles in physical characters. In addition to fibrous willemite, I have also found chrysotile in fine fibers imbedded in the calcite of Mine Hill; it, however, requires but little familiarity with sussexite to distinguish it at a glance from these species.

New Haven, July 18th, 1868.

ART. XXIV.—*On an easy and very effective mode of showing the vibrations in Chladni plates, &c., to a large class, by the use of a calcium or electric lantern;* by JESSE S. CHEYNEY, A. M.

THE lantern is constructed as usual, except that the condensers project some distance in front of the box, which must not be too broad; and the object-glasses are elevated upon a brass stand on the end of a sliding bar two and a half inches wide, which slides smoothly and lightly in a groove cut for it under the bottom of the lantern, and can be clamped in any position required. In my earlier experiments, I used a somewhat different arrangement of the object glasses, but the plan above, which is a modification of one devised by Prof. Albert R. Leeds, of this city, works most satisfactorily.

The lantern thus constructed, is mounted like the telescope of a theodolite, on an axis turning in a pair of vertical posts, which rise out of a circular disc of wood 22 inches in diameter. This disc is fastened loosely at its center, by a bolt with a screw, to the top of the operating table, and thus can be turned completely round in a horizontal plane, and clamped by the screw where required.

On one end of the axis of the lantern, and outside of the upright post is a circle of brass 8 inches in diameter, by which the lantern can be clamped at any angle of elevation or depression by screws set in a pair of brass clamps on the post.

A "square prism," (made either solid of glass or hollow of brass with glass sides and filled with bisulphid of carbon) is mounted so that it can be slipped into the front of the object-glass-mounting and turned upon an axis as usual.

(1.) To show the vibrations in Chladni plates, a narrow clamp, carrying suitable glass plates, is screwed to the upper part of the front of the lantern, so that the center of the plate is directly before the center of the condensers. The lantern is now turned

on its axis until the front is horizontal and the "square prism" slipped into the front of the objective, so that the light is reflected upon the screen. The plate is then focussed, sprinkled with sand, and touched as usual with the bow.

The sudden appearance of the nodal lines is very beautiful. Any figures which can be formed on glass plates can thus be shown with great ease to the largest audience.

The retarding influence of a solution of gum on the sand can be shown in action, and the curves studied in detail.

(2.) "The Magnetic Phantom" can also be most successfully shown in the same way, employing a plate coated with albumen to prevent the filings sliding too much in a body. Numerous other similar experiments, which will occur to every operator, can be readily and beautifully performed.

(3.) The waves produced by striking the surface of mercury in a suitable dish, the colors of thin plates obtained from the action of oils upon water, &c., can be readily shown by depressing the front of the lantern, without the object-glasses, and receiving the light reflected from the dish of fluid upon a proper focussing glass and thus upon the screen.

(4.) In spectrum analysis, the arrangement gives great facility, especially by mounting the prism-box described by J. P. Cooke, Jr.,* so as to slide into the front of the object tube. To keep the edge of the prisms parallel to the slit, a notch is cut in the front of the object tube and a corresponding projection screwed into the sliding end of the prism-box.

(5.) The arrangement proposed has the advantage of simplicity, and gives the greatest facility in the use of the lantern for ordinary purposes as well as in the class of experiments described.

Of course the condensers should not be subjected to the vertical current of hot air from the lime or electric light until they have been thoroughly warmed. With this precaution there is no danger.

I would remark, in conclusion, that this mode of mounting the lantern was devised in the spring, and effected, in all its essential features, in the autumn of 1867; and has been used with great satisfaction in results ever since.

Frankford, Philad., Pa., June 25, 1868.

* This Journal, II, xl, 243, Sept. 1865.

ART. XXV.—*On new specimens of Eozoon Canadense, with a reply to the objections of Professors King and Rowney;* by J. W. DAWSON, LL.D., *and notes by* W. B. CARPENTER, M.D. Illustrated by two plates.

[This paper, published in the Quarterly Journal of the Geological Society for August, 1867, is preceded by an article by Sir Wm. E. Logan, from which the following extracts are made by way of introduction.—EDS.

The most important of these specimens was met with last summer by Mr. G. H. Vennor, one of the assistants on the Canadian Geological Survey, in the township of Tudor and county of Hastings, Ontario, about forty-five miles inland from the north shore of Lake Ontario, west of Kingston. It occurred on the surface of a layer, three inches thick, of dark gray micaceous limestone or calc-schist, near the middle of a great zone of similar rock, which is interstratified with beds of yellowish-brown quartzite, gray close grained siliceous limestone, white coarsely granular limestone, and bands of dark bluish compact limestone and black pyritiferous slates, to the whole of which Mr. Vennor gives a thickness of 1,000 feet. Beneath this zone are gray and pink dolomites, bluish and grayish mica-slates, with conglomerates, diorites, and beds of magnetite, a red orthoclase gneiss lying at the base. The whole series, according to Mr. Vennor's section, which is appended, has a thickness of more than 21,000 feet; but the possible occurrence of more numerous folds than have hitherto been detected, may hereafter render necessary a considerable reduction.

These measures appear to be arranged in the form of a trough, to the eastward of which, and probably beneath them, there are rocks resembling those of Grenville, from which the former differ considerably in lithological character; it is therefore supposed that the Hastings series may be somewhat higher in horizon than that of Grenville. From the village of Madoc, the zone of gray micaceous limestone, which has been particularly alluded to, runs to the eastward on one side of the trough, in a nearly vertical position into Elzivir, and on the other side to the northward, through the township of Madoc into that of Tudor, partially and unconformably overlaid in several places by horizontal beds of Lower Silurian limestone, but gradually spreading, from a diminution of the dip, from a breadth of half a mile to one of four miles. Where it thus spreads out in Tudor it becomes suddenly interrupted for a considerable part of its breadth by an isolated mass of anorthosite rock rising about 150 feet above the general plain, and supposed to belong to the unconformable Upper Laurentian, thus showing that the specimens of *Eozoon* of this neighborhood, like those previously discovered and described, belong to the Lower Laurentian series.

The Tudor limestone is comparatively unaltered; and, in the specimen obtained from it, the general form or skeleton of the fossil (consisting of white carbonate of lime) is imbedded in the limestone, without the presence of serpentine or other silicate, the color of the skeleton contrasting strongly with that of the rock. It does not sink deep into the rock, the form having probably been loose and much abraded on what is now the under part, before being entombed. * * *

In Dr. Dawson's paper mention is made of specimens from Wentworth, and others from Long Lake. In both of these localities the rock yielding them belongs to the Grenville band, which is the uppermost of the three great bands of limestone hitherto described as interstratified in the Lower Laurentian series. That at Long Lake, situated about twenty-five miles north of Côte St. Pierre in the Petite Nation Seigniory, where the best of the previous specimens were obtained, is in the direct run of the limestone there; and like it the Long Lake rock is of a serpentinous character. The locality in Wentworth occurs on Lake Louisa, about sixteen miles north of east from that of the first Grenville specimens, from which Côte St. Pierre is about the same distance north of west, the lines measuring these distances running across several important undulations in the Grenville band in both directions. The Wentworth specimens are imbedded in a portion of the Grenville band, which appears to have escaped any great alteration, and is free from serpentine, though a mixture of serpentine with white crystalline limestone occurs in the band within a mile of the spot. From this gray limestone, which has somewhat the aspect of a conglomerate, specimens have been obtained resembling some of the figures given by Gümbel in his illustrations of the forms met with by him in the Laurentian rocks of Bavaria. * * *

It was the general form on weathered surfaces, and its strong resemblance to *Stromatopora*, which first attracted my attention to *Eozoon;* and the persistence of it in two distinct minerals, pyroxene and loganite, emboldened me, in 1857, to place before the Meeting of the American Association for the Advancement of Science specimens of it as probably a Laurentian fossil. * *

The following is from an ascending section of the Laurentian rocks in the county of Hastings, Ontario, by Mr. H. G. Vennor.

	Feet.
1. Reddish and flesh-colored granitic gneiss	2,000
2. Grayish and flesh-colored gneiss, sometimes hornblendic; mean of several pretty closely agreeing measurements	10,400
3. Crystalline limestone, sometimes magnesian, includes in Elzivir a one foot bed of graphite; sometimes very thin, but in other places attains a thickness of 750 feet; estimated as averaging	400
4. Hornblendic and dioritic rocks, massive or schistose, occasionally associated near the base with dark micaceous schists, chloritic and epidotic rocks, including beds of magnetite; average	4,200
5. Crystalline and somewhat granular magnesian limestone, occasionally interstratified with diorites, and near the base with siliceous slates and small beds of impure steatite	330
This limestone is metalliferous, holding disseminated copper pyrites, blende, mispickel, and iron pyrites, the latter also sometimes in beds of	

	Feet.
two or three feet. Gold occurs in it at the village of Madoc, associated with argentiferous tetrahedrite and in irregular veins with bitter-spar, quartz, and carbonaceous matter.	
6. Gray siliceous or fine-grained mica-slates, with an interstratified mass of about sixty feet of yellowish-white dolomite	400
7. Bluish and grayish micaceous slate, interstratified with layers of gneiss, and occasionally holding crystals of magnetite	500
8. Gneissoid micaceous quartzites, banded gray and white, with a few instratified beds of siliceous limestone	1,900
9. Gray micaceous limestone, sometimes plumbaginous, interstratified below with occasional layers of diorite, and layers of a rusty-weathering gneiss	1,000
This division in Tudor is traversed by numerous N.W. and S.E. veins, holding galena in a gangue of calcite and barite. The Eozoon from Tudor, here described, was obtained from about the middle of this calcareous division, which appears to form the summit of the Hastings series.	
Total thickness	21,130]

I. SPECIMEN OF EOZOON FROM TUDOR, ONTARIO.

This very interesting specimen, submitted to me for examination by Sir W. E. Logan, is, in my opinion, of great importance, as furnishing a conclusive answer to all those objections to the organic nature of *Eozoon* which have been founded on comparisons of its structures with the forms of fibrous, dendritic, or concretionary minerals,—objections which, however plausible in the case of highly crystalline rocks, in which organic remains may be simulated by merely mineral appearances readily confounded with them, are wholly inapplicable to the present specimen.

1. *General appearance.*—The fossil is of a clavate form, six and a half inches in length, and about four inches broad. It is contained in a slab of dark-colored, coarse, laminated limestone, holding sand, scales of mica, and minute grains and fibers of carbonaceous matter. The surface of the slab shows a weathered section of the fossil (Pl. II); and the thickness remaining in the matrix is scarcely two lines, at least in the part exposed. The septa, or plates of the fossil, are in the state of white carbonate of lime, which shows their form and arrangement very distinctly, in contrast to the dark stone filling the chambers. The specimen lies flat in the plane of stratification, and has probably suffered some compression. Its septa are convex toward the broad end, and somewhat undulating. In some places they are continuous half way across the specimen; in other places they divide and re-unite at short distances. A few transverse plates, or connecting columns, are visible; and there are also a number of small veins or cracks passing nearly at right angles to the septa, and filled with carbonate of lime, similar in general appearance to the septa themselves.

On one side, the outline of the fossil is well preserved. The narrow end, which I regard as the basal portion, is rounded. The outline of the side first bends inward, and then outward, forming a graceful double curve, which extends along the greater part of the length. Above this is an abrupt projection, and then a sudden narrowing; and in the middle of the narrow portion, a part has the chambers obliterated by a white patch of carbonate of lime, below which some of the septa are bent downward in the middle. This is probably an effect of mechanical injury, or of the interference of a calc-spar vein.

With the exception of the upper part above referred to, the septa are seen to curve downward rapidly toward the margin, and to coalesce into a lateral wall, which forms the defined edge or limit of the fossil, and in which there are some indications of lateral orifices opening into the chambers. It is worthy of remark that, in this respect, the present specimen corresponds exactly with that which was originally figured by Sir W. E. Logan in the 'Geology of Canada,' p. 49, and which is the only other specimen that has exhibited the lateral limit of the form.

On the side next the matrix, the septa terminate in blunt edges, and do not coalesce; as if the organism had been attached by that surface, or had been broken before being imbedded.

2. *Microscopic characters.*—Under the microscope, with a low power, the margins of the septa appear uneven, as if eroded or tending to an acervuline mode of growth; but occasionally the septa show a distinct and regular margin. For the most part merely traces of structure are presented, consisting of small parts of canals, filled with the dark coloring-matter of the limestone. In a few places (Pl. III, fig. 1), however, these appear as distinct bundles, similar to those in the Grenville specimens, but of fine texture.

[In fig. 2 is represented a portion of the canal-system in a Grenville specimen, in which the canals, which are transparent in one side (being infiltrated with carbonate of lime only) are seen on the other to be partially filled with black matter, probably a carbonaceous residuum of the sarcode which they originally contained.—W. B. C.]

In a few rare instances only can I detect, with a higher power, in the margin of some of the septa, traces of the fine tubulation characteristic of the proper chamber-wall of *Eozoon*. For the most part this seems to have been obliterated by the infiltration of the tubuli with colorless carbonate of lime, similar to that of the skeleton.

In comparing the structure of this specimen with that of

those found elsewhere, it would appear that the chambers are more continuous, and wider in proportion to the thickness of the septa, and that the canal-system is more delicate and indistinct than usual. In the two former respects the specimens from the Calumet and from Burgess approach that now under consideration more nearly than do those from Grenville and Petite Nation; but it would be easy, even in the latter, to find occasional instances of a proportion of parts similar to that in the present example. General form is of little value as a character in such organisms; and so far as can be ascertained, this may have been the same in the present specimen and in that originally obtained from the Calumet, while in the specimens from Grenville a massive and aggregative mode of growth seems to have obliterated all distinctness of individual shape. Without additional specimens, and in the case of creatures so variable as the Foraminifera, it would be rash to decide whether the differences above noticed are of specific value, or depend on age, variability, or state of preservation. For this reason I refer the specimen for the present to *Eozoon Canadense*, merely distinguishing it as the Tudor variety.

From the state of preservation of the fossil, there are no crystalline structures present which can mislead any ordinarily skilful microscopist, except the minute veins of calcareous spar traversing the septa, and the cleavage-planes which have been developed in some portions of the latter.

I would remark that, as it seemed desirable not to injure any more than was absolutely necessary a unique and very valuable specimen, my observations of the microscopic structure have been made on a few slices of small size,—and that, as the microscopic structures are nearly the same in kind with those of specimens figured in former papers, I have not thought it necessary to prepare numerous drawings of them; while the admirable photograph executed for Sir W. E. Logan by Mr. Notman illustrates sufficiently the general form and arrangement of parts (see Pl. II).

3. *Concluding remarks.*—In a letter to Dr. Carpenter, quoted by him in the 'Quarterly Journal of the Geological Society' for August, 1866, p. 228, I referred to the occurrence of *Eozoon* preserved simply in carbonate of lime. The specimens which enable me to make that statement were obtained at Madoc, near Tudor, this region being one in which the Laurentian rocks of Canada appear to be less highly metamorphosed than is usual. The specimens from Madoc, however, were mere fragments, imbedded in the limestone, and incapa-

ble of showing the general form. I may explain, in reference to this, that long practice in the examination of these limestones has enabled me to detect the smallest fragments of *Eozoon* when present, and that in this way I had ascertained the existence of this fossil in one of the limestones of Madoc before the discovery of the fine specimen now under consideration.

I am disposed to regard the present specimen as a young individual, broken from its attachment and imbedded in a sandy calcareous mud. Its discovery affords the hope that the comparatively unaltered sediments in which it has been preserved, and which also contain the worm-burrows described by me in the 'Quarterly Journal of the Geological Society' for November,* will hereafter still more largely illustrate the Laurentian fauna.

II. SPECIMENS FROM LONG LAKE AND WENTWORTH.

Specimens from Long Lake, in the collection of the Geological Survey of Canada, exhibit white crystalline limestone with light green compact or septariiform† serpentine, and much resemble some of the serpentine-limestones of Grenville. Under the microscope the calcareous matter presents a delicate areolated appearance, without lamination; but it is not an example of acervuline *Eozoon* but rather of fragments of such a structure, confusedly aggregated together, and having the interstices and cell-cavities filled with serpentine. I have not found in any of these fragments a canal-system similar to that of *Eozoon Canadense*, though there are casts of large stolons, and, under a high power, the calcareous matter shows in many places the peculiar granular or cellular appearance which is one of the characters of the supplemental skeleton of that species. In a few places a tubulated cell-wall is preserved, with structure similar to that of *Eozoon Canadense.*

Specimens of Laurentian limestone from Wentworth, in the collection of the Geological Survey, exhibit many rounded siliceous bodies, some of which are apparently grains of sand, or small pebbles; but others, especially when freed from the calcareous matter by a dilute acid, appear as rounded bodies, with rough surfaces, either separate or aggregated in lines or groups, and having minute vermicular processes projecting from their surfaces (Pl. III, fig. 3). At first sight these suggest the idea of spicules; but I think it on the whole more

* Vol. xxii, p. 608.

† I use the term 'septariiform' to denote the *curdled* appearance so often presented by the Laurentian serpentine.

likely that they are casts of cavities and tubes belonging to some calcareous Foraminiferal organism which has disappeared.

Similar bodies, found in the limestone of Bavaria, have been described by Gümbel, who interprets them in the same way.* They may also be compared with the siliceous bodies mentioned in a former paper as occurring in the loganite filling the chambers of specimens of *Eozoon* from Burgess.

III. SPECIMENS FROM MADOC.

I have already referred to fragments of *Eozoon* occurring in the limestone at Madoc, one of which, found several years ago, I did not then venture to describe as a fossil. It projected from the surface of the limestone, being composed of a yellowish dolomite, and looking like a fragment of a thick shell. When sliced, it presents interiorly a crystalline dolomite, limited and separated from the enclosing rock by a thin wall having a granular or porous structure, and excavated into rounded recesses in the manner of *Eozoon.* It lies obliquely to the bedding, and evidently represents a hollowed flattened calcareous wall filled by infiltration. The limestone which afforded this form was near the beds holding the worm-burrows described in the Society's Journal for Nov. 1866.

[A thin section of this body, carefully examined microscopically, presents numerous and very characteristic examples of the canal-system of *Eozoon*, exhibiting both the large widely branching systems of canals and the smaller and more penicillate tufts (Pl. III, figs. 4, 5) shown in the most perfect of the serpentinous specimens—but with this difference, that the canals, being filled with a material either identical with or very similar to that of the substance in which they are excavated, are so transparent as only to be brought into view by careful management of the light.—W. B. C.]

IV. OBJECTIONS TO THE ORGANIC NATURE OF EOZOON.

The discovery of the specimen from Tudor, above described, may appear to render unnecessary any reference to the elaborate attempt made by Profs. King and Rowney to explain the structures of *Eozoon* by a comparison with the forms of fibrous and dendritic minerals,† more especially as Dr. Carpenter has already shown their inaccuracy in many important points. I think, however, that it may serve a useful purpose shortly to point out the more essential respects in which this comparison fails with regard to the Canadian specimens—with the view of relieving the discussion from matters irrelevant to it, and of

* Proceedings of Royal Academy of Munich, 1866; Q. J. G. S., vol. xxii, pt. i. p. 185 *et seq.*; also Can. Naturalist, vol. iii, p. 81.

† Quart. Journ. Geol. Soc., vol. xxii, pt. ii, p. 23.

fixing more exactly the limits of crystalline and organic forms in the serpentine-limestones and similar rocks.

The fundamental error of Messrs. King and Rowney arises from defective observation—in failing to distinguish, in the Canadian limestones themselves, between organic and crystalline forms. This is naturally followed by the identification of all these forms, whether mineral or organic, with a variety of purely crystalline arrangements occurring in other rocks, leading to their attaching the term 'Eozoonal' to any rock which shows any of the characters, whether mineral or organic, thus arbitrarily attached to the Canadian *Eozoon.* This is obviously a process by which the structure of any fossil might be proved to be a mere *lusus naturæ.*

A notable illustration of this is afforded by their regarding the veins of fibrous serpentine, or chrysotile, which occur in the Canadian specimens, as identical with the tubulated cell-wall of *Eozoon*,—although they admit that these veins traverse all the structures indifferently, and do not conform to the walls of the chambers. But any microscopist who possesses specimens of *Eozoon* containing these chrysotile veins may readily satisfy himself that, under a high power, they resolve themselves into *prismatic crystals in immediate contact with each other;* whereas, under a similar power, the true cell-wall is seen to consist of *slender, undulating, rounded threads of serpentine, penetrating a matrix of carbonate of lime.* Under polarized light, more especially, the difference is conspicuously apparent. It is true that, in many specimens and parts of specimens, the cell-wall of *Eozoon* is badly preserved and fails to show its structure ; but in no instance does it present the appearance of chrysotile, or of any other fibrous mineral, when examined with care under sufficiently high powers. In my original examination of Sir William Logan's specimens from Grenville and the Calumet, I did not detect the finely tubulated cell-wall which is very imperfectly preserved in those specimens ; but the veins of fibrous serpentine were well known to me ; and when Dr. Carpenter discovered the tubulation of the cell-wall in the specimens from Petite Nation, I compared this structure with that of these veins, and satisfied myself of its distinctness before acceding to his conclusions on this point.

It would also appear that the radiating and sheaf-like bundles of crystals of tremolite, or similar prismatic minerals, which occur in the Canadian serpentines, and also abound in those of Connemara, have been confounded with the tubulation of *Eozoon ;* but these crystals have no definite relation to the forms of that fossil, and often occur where these are en-

tirely absent; and in any case they are distinguished by their straight prismatic shape and their angular divergence from each other. Much use has also been made of the amorphous masses of opaque serpentinous matter which appear in some parts of the structure of *Eozoon*. These I regard as, in most cases, simply results of alteration or defective preservation, though they might also arise from the presence of foreign matters in the chambers, or from an incrustation of mineral matter before the final filling up of the cells. Generally their forms are purely inorganic; but in some cases they retain indications of the structures of *Eozoon*.

With reference to the canal-system of *Eozoon*, no value can be attached to loose comparisons of a structure so definite with the forms of dendritic silver and the filaments of moss-agates; still less can any resemblance be established between the canal-system and vermicular crystals of mica. These occur abundantly in some serpentines from the Calumet, and might readily be mistaken for organic forms; but their rhombic or hexagonal outline when seen in cross section, their transverse cleavage planes, and their want of any definite arrangement or relation to any general organic form, are sufficient to undeceive any practised observer. I have not seen specimens of the metaxite from Reichenstein referred to by Messrs. King and Rowney; but it is evident, from the description and figure given of it, that, whether organic or otherwise, it is not similar to the canals of *Eozoon Canadense*. But all these and similar comparisons are evidently worthless when it is considered that they have to account for definite, ramifying, cylindrical forms, penetrating a skeleton or matrix of limestone, which has itself a definite arrangement and structure, and, further, when we find that these forms are represented by substances so diverse as serpentine, pyroxene, limestone, and carbonaceous matter. This is intelligible on the supposition of tubes filled with foreign matters, but not on that of dendritic crystallization.

If all specimens of *Eozoon* were of the acervuline character, the comparisons of the chamber-casts with concretionary granules might have some plausibility. But it is to be observed that the laminated arrangement is the typical one; and the study of the larger specimens, cut under the direction of Sir W. E. Logan, shows that these laminated forms must have grown on certain strata-planes before the deposition of the overlying beds, and that the beds are, in part, composed of the broken fragments of similar laminated structures. Further, much of the apparently acervuline *Eozoon* rock is composed of such broken fragments, the interstices between which should not be confounded with the chambers; while the fact

that the serpentine fills such interstices as well as the chambers shows that its arrangement is not concretionary.* Again, these chambers are filled, in different specimens, with serpentine, pyroxene, loganite, calcareous spar, chondrodite, or even with arenaceous limestone. It is also to be observed that the examination of a number of limestones, other than Canadian, by Messrs. King and Rowney, has obliged them to admit that the laminated forms in combination with the canal-system are 'essentially Canadian,' and that the only instances of structures clearly resembling the Canadian specimens are afforded by limestones Laurentian in age, and in some of which (as, for instance, in those of Bavaria and Scandinavia) Carpenter and Gümbel have actually found the structure of *Eozoon*. The other serpentine-limestones examined (for example, that of Skye) are admitted to fail in essential points of structure; and the only serpentine believed to be of eruptive origin examined by them is confessedly destitute of all semblance of *Eozoon*. Similar results have been attained by the more careful researches of Prof. Gümbel, whose paper is well deserving of study by all who have any doubts on this subject.

In the above remarks I have not referred to the disputed case of the Connemara limestones; but I may state that I have not been able to satisfy myself of the occurrence of the structures of *Eozoon* in such specimens as I have had the opportunity to examine.† It is perhaps necessary to add that there exists in Canada abundance of Laurentian limestone which shows no indication of the structures of *Eozoon*. In some cases it is evident that such structures have not been present. In other cases they have been obliterated by processes of crystallization. As in the case of other fossils, it is only in certain beds, and in certain parts of those beds, that well-characterized specimens can be found. I may also repeat here that in the original examination of *Eozoon*, in the spring of 1864, I was furnished by Sir W. E. Logan with specimens of all these limestones, and also with serpentine-limestones of Silurian age, and that, while all possible care was taken to compare with the specimens of *Eozoon*, it was not thought necessary to publish notices of the crystalline and concretionary forms observed, many of which are very curious, and might afford materials for other papers of the nature of that criticised in the above remarks.

* I do not include here the 'septariiform' structure referred to above, which is common in the Canadian serpentine and has no connection with the forms of the chambers.

† Such Irish specimens of serpentine limestone as I have seen, appear much more highly crystalline than the beds in Canada which contain *Eozoon*.

[The examination of a large number of sections of a specimen of *Eozoon*, recently placed in my hands by Sir William Logan, in which the canal-system is extraordinarily well preserved, enables me to supply a most unexpected confirmation of Dr. Dawson's statements in regard to the occurrence of dendritic and other forms of this system, which cannot be accounted for by the intrusion of any foreign mineral; for many parts of the calcareous lamellæ in these sections, which, when viewed by ordinary transmitted light, appear quite homogeneous and structureless, are found, when the light is reduced by Collin's 'graduating diaphragm,' to exhibit a most beautiful development of various forms of canal-system (often resembling those of Dr. Dawson's Madoc specimen, represented in Pl. III, figs. 4, 5), which cross the cleavage planes of the shell-substance in every direction. Now these parts, when subjected to decalcification, show no trace of canal-system; so that it is obvious, both from their optical and from their chemical reactions, that the substance filling the canals must have been *carbonate of lime*, which has thus completely solidified the shell layer, having been deposited in the canals previously excavated in its interior, just as crystalline carbonate of lime fills up the reticular spaces of the skeleton of Echinodermata fossilized in a calcareous matrix. This fact affords conclusive evidence of *organic structure*, since no conceivable process of crystallization could give origin to dendritic extensions of carbonate of lime disposed on exactly the same crystalline system with the calcite which includes it, the two substances being mineralogically homogeneous, and only structurally distinguishable by the effect of their junction-surfaces on the course of faint rays of light transmitted through them.—W. B. C.]

Explanation of the Plates.

Plate II.

Specimen of *Eozoon Canadense*, imbedded in a dark-colored homogeneous limestone occurring in the Lower Laurentian series in Tudor, Ontario; two-thirds of the natural size.

Plate III.

Fig. 1. Section of one of the calcareous layers of the Tudor specimen (Plate II), showing canal-system imperfectly infiltrated with black (carbonaceous?) matter; magnified 120 diameters.

2. Section of the shelly layer of a specimen of *Eozoon* from Grenville, showing a minute form of canal-system partly injected with b partly with serpentine; magnified 120 diameters.

3. Siliceous bodies (internal casts?) from a specimen of *Eozoon* from Wentworth; magnified 50 diameters.

4, 5. Sections of a fragment of *Eozoon* from the Madoc limestone, showing various forms of canal-system filled with carbonate of lime; magnified 120 diameters.

ART. XXVI.—*On Aquacreptite, a new mineral, and on Corundophilite of Chester;* by CHARLES UPHAM SHEPARD.

1. *Aquacreptite.*—The substance here named *aquacreptite*, on account of the crackling sound it emits when thrown into water, was sent me last autumn by Mr. W. W. Jefferis, of West Chester, Pa., with the remark that it was found at that place in a narrow seam in serpentine. The specimens are in little polyhedral fragments of the size of hazel-nuts, with flat or concave surfaces, somewhat resembling in shape the miemite from Miemo in Tuscany. Whether the mineral is an original compound, or an oxydated hydration of another substance, and if so, of what species, it is difficult to say. At present, however, it is entirely homogeneous in aspect, and possessed of much uniformity of composition. It possesses the following characters:

Massive. H.=2·5. Gr.=2·08 (Shep.), 2·05 (Eaton). Color yellowish brown. Streak orange yellow. Dull. Brittle. Fracture small conchoidal. Adheres rather feebly to the tongue. Falls asunder in water with a crackling (decrepitating) noise. The decrepitation lasts only for a moment, and is more striking if the water is warm. It does not wholly fall to powder in water; but a portion of the mass still retains a feeble degree of cohesion.

Ignited in powder, it loses 23 p. c. without change of color. Previous to ignition it is taken up by aqua regia, with exception of the silica, which separates in flocks.

Three analyses were made; 1, by Prof. James H. Eaton of Beloit College, Wisconsin; 2, by Mr. Henry C. Humphrey, a student of Amherst College, and the third by myself.

	1.	2.	3.
Silica,	43·03	41·56	41·00
Magnesia,	19·58	(not determined)	17·60
Peroxyd iron,	12·30	12·45	13·30
Alumina,	5·56	6·71	4·00
Water,	17·40	16·00	23·00
	97·87		98·90

1. *Corundophilite.*—As there had existed some perplexity in reference to this mineral as found at Chester, Mass., as the abundant associate of the emery and a constituent also of the talcose slates embracing the emery vein, I requested Prof. Eaton (then in charge of the Amherst College laboratory) to analyze the two leading varieties of the corundophilite, viz: (1) that in crystals accompanied by diaspore, rutile and sapphire

crystals, and (2) the more common variety which occurs in scales (slightly columnar), forming a narrow seam on two sides of a vein of finely granular indianite. The latter variety, as it runs for some distance in proximity to the eastern wall of the emery, has been called the "*fringe rock*" by the workmen.

Var. 1, is the same as that analyzed by Pisani, to whom also I furnished the specimens upon which he operated, and whose results I here introduce for comparison with those obtained by Eaton.

Gr. of crystallized variety as determined by Eaton =2·83.

	1.		2.
	(Eaton.)	(Pisani.)	Eaton.
Silica,	24·77	24·0	24·69
Alumina,	25·52	25·9	28·52
Magnesia,	21·88	22·7	21·86
Protoxyd iron,	15·19	14·8	16·38
Water,	11·98	11·9	(not determined.)
	99·34	99·3	

It is very noticeable that wherever at this locality the corundophilite enters into the talcose or micaceous schist it presents characters easily distinguishing the compound from all the known varieties of chlorite slate, producing in fact a new rock, whose appropriate designation will be *corundophilite slate*.

Amherst College, July 22, 1868.

Art. XXVII.—*A new locality of Meteoric Iron in Georgia*; by Charles Upham Shepard.

I am indebted to the kindness of Prof. Charles H. Hitchcock both for a knowledge of the present meteorite and for assistance in the purchase of the same from its original proprietor. It was ploughed up in April last on the farm of Mr. Michael Sullivan, two and a half miles southwest of Losttown, Cherokee county, Georgia.

It weighs six pounds and ten ounces, and has very strikingly the form of a human foot. Its color is almost perfectly black, and is wholly free from stains of iron-rust. It evinces no tendency to exfoliation; nor is it uniformly covered by a fused coating. Widmannstättian figures are visible directly in one portion of the surface. The indentations are broad and shallow, though on the whole, well pronounced.

A thin slice weighing twenty-seven grams was sawn from the heel end of the mass. The hardness proved uniform, no py-

rites having been encountered in the section. The specific gravity of the fragment is 7·52. On being etched with a dilute nitric acid, very beautiful Widmannstättian figures were presented, not quite identical with any with which I am acquainted, but most nearly resembling those of the Seneca Lake iron—the difference between the two consisting mainly in a less breadth to the bars by about one-third, in the former of these irons.

I have thus far found time only to examine the filings (or rather sawings) of this iron for sulphur and nickel. The first is wholly wanting, while the latter is abundantly present.

Amherst College, July 21, 1868.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Hinrichs's Atom Mechanics.*—That the readers of this Journal may judge of the estimation in which Prof. Hinrichs's work on Atom-mechanics is held abroad, we translate from the German of Professor H. Fleck, of Dresden, the following notice, the original of which will be found in the *Jahrbuch für Mineralogie, Geologie*, etc., 1868, p. 333. W. G.

Under the title of "*Atom-mechanics, or Chemistry a Mechanics of the Pan-atoms*," Mr. Gustav Hinrichs, Professor of Physics, Chemistry and Mineralogy in the University of Iowa, in the United States, has published and multiplied by metallography a monograph, in which chemical processes are based upon mechanical principles, and according to which an attempt is made to refer the atomic groupings of the elements to mechanical phenomena of motion. The solution of this important problem, with which the author has been busied for twelve years, depends upon the hypothesis of an atomic system entirely new in its details, the starting point of which is *pantogen*, the fundamental principle of all chemical elements. The chemically active atoms of the elements, by the association of which chemical compounds are finally produced, are formed by the grouping of these pantogen-atoms or pan-atoms. This hypothesis it is true is not new, since in its fundamental idea it was employed by Leibnitz, Lotze, v. Wolff, Herbart and others, and has recently received from Fechner a foundation so solid and philosophically concrete, that serious doubts of the existence of primary material atoms, whether we term them monads or pan-atoms, can scarcely again be raised. But the mode of conceiving the groups of atoms as Mr. Hinrichs offers them to our consideration, with the added introduction into the calculations of the atomic weights adopted in science, is new and in every respect peculiar. Upon these last the author bases the whole essence of the phenomena of chemical change, and with their recognition Mr. Hinrichs will unquestionably obtain the reputation of a second Kepler.

The reader will have no difficulty in judging how far Mr. Hinrichs's mode of conception deserves such a recognition if we endeavor in what follows to elucidate the Atom-mechanics from the standing point of the exact sciences. After presenting the pan-atoms as probably existing in a free state in the extreme outer atmosphere of the sun and thus as producing light, and considering hydrogen as standing nearest to pantogen, Mr. Hinrichs expresses himself as follows: There are but two possible modes of combination for equal material points in one plane, *i. e.*, as angle-points of an equilateral triangle or of a square.

Accordingly there are but two kinds of pantogen-compounds or elements, trigonoids and tetragonoids.

This expression requires in the first place an explanation; by it Mr. Hinrichs represents the pantogen atoms as points which take their places at the angles of triangles or squares, by the association of which panatomic surfaces or atomic areas are formed, which again when superposed upon each other in number proportioned to the atomic weight of the element form the atom of an element. By this however we are involuntarily brought to the following questions, no answer to which is offered:

1. Does not Mr. Hinrichs know that the square itself may be made up of two triangles, and that consequently his tetragonoids may be referred to trigonoids?

2. If the pantogen elements form angle points in a plane, what fills up the plane and the intermediate spaces formed by the superposition of the planes? But admitting that these questions in their answers do not tell to the disadvantage of Mr. Hinrichs's ideas, the following considerations present insuperable objections: Mr. Hinrichs introduces in his calculations the atomic weights of the elements, taken double or quadruple according to the necessity of the case, under the name of Hinrichs's atomic weights, by saying that the atomic weight corresponds to the number of pantogen atoms in an elementary atom, so that if we have for instance an element with the atomic weight 100, 100 pantogen atoms are necessary for the formation of this element. These 100 atoms are distributed upon m-surfaces, in each of which there are n-pantogen points, so that $m \times n$ must be the atomic weight of the element.

Now as the author considers the trigonoids (non-metallic elements) as arising from triangles which by their juxtaposition produce an atomic surface and by their superposition a trigonoid atom, while the tetragonoid atoms (metals) are assumed to arise from the superposition of square surfaces, he fails to see the trifling weakness of making from the atomic weight of fluorine = 38, the number 35 = 5 surfaces with 7 points; from the atomic weight of bromine = 160, the number 156 = 12 surfaces each with 13 pan-atoms; from the atomic weight of arsenic =150 the number 152 = 8 surfaces each with 19 pan-atoms, &c., and consequently of arbitrarily altering, increasing or diminishing, as suits the figures which he has invented for the benefit of his hypothesis, the combining weights of the elements fixed by the most precise scientific

labors. If however, for the sake of a good idea we allow this arbitrary treatment of tested and recognized numerical values to pass unchallenged, we must abandon all hope and faith in it when we come to the hydrogen-atom. This element, which with its lowest atomic weight can consist at the utmost of only 4 pan-atoms, belongs to the tetragonoids and furnishes therefore only a surface but no body. Hydrogen is therefore, as the normal magnitude of Hinrichs's atomic weights most unkindly treated, and moves about in the world as a surface and without bodily attributes. This reduction of his system to a surface suggests a great superficiality in its treatment, and removes from the mind of the critic all further doubts as to the signification and value of Hinrichs's atom-mechanics. While apparently based upon mathematical principles, Mr. Hinrichs's work dispenses with a mathematical basis; though furnished with an array of chemical atomic weights, these display no true scientific treatment; though adorned with physical phrases, correct ideas of the relations of space and force are entirely wanting. Science will consequently, have nothing to expect from Prof. Hinrichs's work in its present form, and like a still-born child it will be neither mourned nor missed. Every one who has seen from the above the weak foundation upon which the fantastic air-castle of this idea of pantogen is erected, will think it most advisable to be silent as to the further development of Prof. Hinrichs's monograph in the application and utilization of his ideas.

2. *On Vortex-rings in Air;* by Professor ROBERT BALL.—The production of vortex-rings in air is experimentally shown by the interesting phenomena produced by Professor Tait. These are described by Sir William Thompson in the Philosophical Magazine for July 1867. I repeated these experiments with the kind aid of Dr. Emerson Reynolds; in doing so an interesting variation of them presented itself which renders the air-rings actually visible.

While engaged on this subject, Mr. Yeates suggested to me to try the effect of discharging an air-ring at a column of smoke; this was done, and a most curious and unexpected appearance was the result. The box employed was a 2-feet cube, and had a hole 8 inches in diameter, the side opposite having a piece of stout sacking strained over it. A blow on the sacking causes an air-ring to dart from the hole. Two large flasks containing respectively hydrochloric acid and ammonia were arranged with their mouths in close proximity and their contents vigorously boiled; from the union of their vapors a dense column of the fumes of chlorid of ammonium ascended. This apparatus was placed about eight or ten feet in front of the box. An air-ring on first leaving the box was of course invisible; when it reached the column of smoke it could be seen to force its way through it; but when it left the column and during its subsequent existence its appearance was very remarkable. At first the idea suggested itself that the single air-ring had formed two concentric smoke-rings; but closer examination and an improved column of smoke showed what had really occurred. The air-ring had penetrated the smoke quite uninjured; it had not

apparently left any of its particles behind, nor had it admitted an atom of smoke into it; but it had drawn with it sufficient of the smoke to form a complete shell, which enclosed it, and thus rendered the air visible. The phenomenon is quite in accordance that conservative tendency which theory shows must belong to a vortex-filament. The appearance is one of great beauty, and suggested the name "negative smoke-ring."

It was considered desirable to make these experiments before an evening scientific meeting of the Royal Dublin Society. Smoke-rings are seen to greatest perfection when directed along a sunbeam admitted into an otherwise darkened room; and though they can be seen beautifully in ordinary diffused sunlight, by gaslight they make a very poor show; special means of illumination are therefore necessary. The following arrangement is found to succeed admirably. By a simple optical contrivance the radiation from a brilliant lime-light is concentrated into a slightly conical beam, all other light in the room being extinguished. The box is placed in the beam, the orifice facing the lamp, and from twenty to thirty feet distant from it; at the box the diameter of the circular section of the luminous cone is from 3 to 4 feet. A smoke-ring driven from the box will, if suitably directed, traverse the beam through its entire length until it reaches the lamp; and as it is brilliantly illuminated throughout its path, the appearance presented is of great beauty. A second box can be placed at the lamp end of the beam, so as to show the effect of the collision of rings. The fumes produced by burning a small piece of phosphorus develops an ample supply of smoke in the interior of the box. This mode of producing smoke was suggested to me by Dr. William Barker; it is simpler to manage inside the box than the apparatus necessary for forming chlorid of ammonium. By the introduction of colored glasses various pleasing effects can be produced. If a sheet of tissue-paper be very lightly attached to a frame and interposed in the beam, a vigorously sent smoke-ring will sweep it away in a striking manner.

To exhibit the air-rings, the column of smoke already described is placed so as to ascend through the beam, and rather nearer the box than the lamp. An air-ring from the box is, of course, invisible till it reaches the column, while in its passage from the column to the lamp the curious phenomenon already mentioned is most beautifully shown. The negative smoke-rings are much better seen when thus illuminated than by ordinary daylight. A box of the dimensions previously given, viz: a 2-foot cube with a hole of 8 inches diameter, was found to answer better for this purpose than a somewhat smaller box with a hole of 6 inches aperture.

The column of smoke may perhaps serve as a test for the existence of vortex-rings in other cases, when, produced in air, they are invisible. This was tried in one instance. It is a common trick to blow out a candle by the puff of air from the muzzle of a gun when a percussion-cap is exploded on the nipple. This puff is probably a vortex-ring. Owing to the high velocity with which it

moves, it is difficult to trace the effect produced by the column of smoke. One person, however, firing from an elevated position and at a distance of fifteen or twenty feet from the flasks down upon the column, and another watching against a dark background, a distinct ring was occasionally seen to dart from the column after the percussion of a cap; but whether these were "negative rings," as was expected, or ordinary "smoke-rings," was not easy to determine.

Royal College of Science for Ireland, Dublin, May 28, 1868.

P. S.—Since writing the foregoing, it occurred to me to fill the box with ammoniacal gas and to discharge rings from this at a column of the vapor of hydrochloric acid. In this case the ring, before reaching the column, is perfectly invisible, and the existence of the column is only seen by slight traces of partially condensed vapor. As was expected, a beautiful ring appeared, from the combination of the two gases, when the ammonical ring reached the column.—*Phil. Mag.*, IV, xxxvi, 12, July, 1868.

June 16, 1868.

II. MINERALOGY AND GEOLOGY.

1. *Twentieth Annual Report of the Regents of the University of the State of New York on the condition of the State Cabinet of Natural History and the Historical and Antiquarian Collections annexed thereto.* Transmitted to the Legislature, April 15th, 1867. 410 pp. 8vo, with 20 plates. Albany. 1868.—This State Cabinet Report for 1867 is mainly occupied by the results of the paleontological researches of Prof. Hall. A large number of new paleozoic species are described, along with much that is of interest with regard to the structure and the relations of genera. Prof. Hall has here reproduced his very important memoir on Graptolites published by the Canadian Survey, with some additional observations.

In connection with a statement of his observations on the shell structure of some species of Spirifera, on p. 256, Prof. Hall takes occasion to complain of Mr. F. B. Meek who has published on the same subject. Mr. Meek's paper appeared in this Journal, as stated, in May, 1866. Mr. Hall, in a letter written subsequently to one of the editors of this Journal, claimed that he had had the same views for some time in print, and to show this sent on a few pages of his unpublished 4th volume. In reply, it was stated that *printing* without publishing, was no basis for a claim in science. Mr. Hall afterward had the chapter issued in the Proceedings of the American Philosophical Society, but issued without the slightest allusion to Mr. Meek's earlier publication. Mr. Meek in a later article, May, 1867, alludes in a note to this fact. Mr. Hall in reply urges that his article in the Proceedings was not published as a *paper* presented to the Philosophical Society, but as an extract from a "volume printed and not yet published," and implies that therefore he was under no obligations to allude to Mr. Meek. He

adds "that the Journal of Science professes to deal *fairly and justly* in all scientific matters, and one of its editors was in possession of all the facts and could have prevented all cause of ill feeling on the part of any one." But the editors of the Journal cannot but think that if Mr. Hall had dealt fairly in the first case, and alluded, in connection with the article in the Proceedings of the Philosophical Society, to Mr. Meek's prior publication, as he ought to have done, there would have been no occasion for any ill-feeling on the part of any one. An article published by a Society is a *paper* presented for publication, whatever may have been its previous history, and comes under the ordinary rule of courtesy among authors.

We may add that there is no man of science in the country who, in our opinion, aims to be more strictly just to others, and is more faithful in research, than Mr. Meek.

2. *On a new large Enaliosaur;* by Prof. E. D. Cope.—Prof. Cope exhibited to the Academy several fragments of a large Enaliosaur, discovered by the Academy's correspondent at Fort Wallace, Kansas, Dr. Theophilus H. Turner. Portions of two vertebræ, brought east by Dr. LeConte from his geological survey of the Pacific Railroad route, had previously indicated to the speaker the existence of an animal related to the Plesiosaurus; and the recovery of the greater part of the reptile had confirmed this affinity.

The remains consisted of over one hundred vertebræ, with numerous portions of ribs, the greater part of the pelvic and scapular arches, with two long bones somewhat like femora. Part of a muzzle, with teeth, belonged to the same animal.

The species represented a genus differing in important features from Plesiosaurus and its near allies. These were the absence of diapophyses on the caudal vertebræ, and the presence of inferiorly directed plate-like parapophyses, which took the place of the usual chevron bones in the same position; also in the presence of chevron-like bones on the inferior surfaces of the cervical vertebræ; further in some details of the scapular and pelvic arches. The diapophyses of the dorsal vertebræ originated from the centrum, and not from the neural arch.

In generic features it is related to the Cimoliosaurus and Brimosaurus of Leidy, so far as the latter are yet known. It differed from both of them in lacking diapophyses on the lumbar vertebræ.

The general form was different from Plesiosaurus in the enormous length of the tail, and the relatively shorter cervical region. The total length of the vertebral column sent was thirty-one feet ten inches, divided as follows: caudals 18 ft. 10 in., dorsals 9 ft. 8 in., cervical 3 ft. 4 in.; adding for missing cervicals and cranium at least 2 ft. 6 in., we have a total of 34½ feet. An interval of three to four feet occurred between the cervicals and dorsals as they lay in the cliff from which they were excavated, which if, as is probable, it was occupied by vertebræ in the animal, would give a length of thirty-eight feet. The caudal vertebræ had very compressed centra, and elevated neural and hæmal laminæ, and were of unusually elongate form. Neural arches everywhere on the column co-ossified.

All the vertebræ considerably more constricted medially than in Brimosaurus or Cimoliosaurus, and none except cervicals with such small antero-posterior diameter as the latter possess.

The general characters of the species are to be presented in a special essay.

He called it *Elasmosaurus platyurus* Cope, from the caudal laminæ, and the great plate bones of the sternal and pelvic regions. It was a marine saurian, whose progression was more largely accomplished by its tail than by its paddles.

The teeth and muzzle showed it to be an ally of Plesiosaurus. The former were cylindric, implanted in very deep alveolæ, and furnished with a very small pulp cavity. The exposed surface closely and sharply striate to the narrowly acuminate tip.

The beds were argillaceous, with much gypsum; the latter mineral coating the bones. The age was Cretaceous; perhaps, according to LeConte, the upper middle. The matrix beneath the dorsal vertebræ contained remains of perhaps six species of fishes, several Ctenoid, among them a known Enchodus, and a Sphyraæa, to be called *Sph. carinata* Cope.—*Proc. Acad. Sci. Philad.*, 1868, 92.

3. *Bone caves of Brazil and their animal remains;* by Prof. Reinhardt.—An important though popular paper by Prof. Reinhardt, on the bone caves of Brazil, closes with the following general conclusions:

"1. During the Post-pliocene epoch, Brazil was inhabited by a very rich Mammalian Fauna, of which the recent one might almost be said to be a mere fraction or a crippled remnant, as many of its genera, even families and sub-orders, have vanished, and very few have been added in more recent times.

2. During the whole Post-pliocene epoch the Brazilian Mammalian Fauna had the same peculiar character which now distinguishes the South American Fauna, compared with that of the old world; the extinct genera belonging to groups and families, that to this very day are peculiarly characteristic of South America. Only two of its genera, the one extinct (Mastodon), the other still living (the Horse), belong to families that in our epoch are limited to the Eastern Hemisphere.

3. All the Mammalian orders were not in the same degree richer in genera in former times than now. The Bruta, Ungulata, Proboscidea, and, lastly, the Feræ, have relatively suffered the greatest losses. Some orders, for instance the Chiroptera and Simiæ, number perhaps even more genera now than formerly.

4. The Post-pliocene Mammalian Fauna of South America differed much more from the modern one, and was especially more rich in peculiar genera, now extinct, than the corresponding fauna of the Old World.

5. The scantiness of great Mammalia—one might say the dwarf-like stamp impressed upon the South American Mammalian Fauna of our days, when compared with that of the Eastern Hemisphere, was much less observable, or rather did not exist in the prehistoric Fauna. The Post-pliocene Mastodonts, Macrauchenìæ, and Toxo-

donts of Brazil, its many gigantic Armadillos and Sloths could well rival the Elephants, Rhinoceros, and Hippopotami, which during the same period roamed the soil of Europe.—*Tidschrift f. pop. Fremstillinger af Nat.*, 1867.—*The Geological Magazine, or Monthly Journ. of Geology*, p. 227.

4. *Paleozoic Entomostraca.*—Prof. T. RUPERT JONES and Dr. H. B. HOLL, publish No. VIII of notes on the Paleozoic Bivalved Entomostraca, in the Annals and Magazine of Nat. History, IV, ii, 54, (July, 1868), describing some Lower Silurian species from the chair of Kildare, Ireland.

5. *On the discovery of a new Pulmonate Mollusk* [*Zonites* (*Conulus*)* *priscus* Cpr.] *in the Coal-formation of Nova Scotia; by* J. W. DAWSON, LL.D., *with a description of the Species; by* PHILIP P. CARPENTER, M.D., (Q. J. Geol. Soc., Nov. 1867, 331.)—This memoir describes a shell found last summer in the course of excavations made under the direction of Dr. Dawson in the bed in subdivision VIII of the Joggins Section, between coals No. 37 and No. 38 of Logan's sectional list, already referred to in previous papers† as containing great numbers of shells of *Pupa vetusta.* This bed is 1217 feet below that in which *Pupa vetusta* was originally discovered in trunks of erect Sigillariæ, and about 42 feet below coal No. 37,‡ or nearly in the middle of the band of reddish and gray sandstones and shales intervening between coals No. 37 and No. 38.

In digging into the bed, it was found that the shells of *Pupa* were irregularly disposed in nests, and were in some spots very abundant, especially in the argillaceous and nodular parts, while in other places, and especially in the more carbonaceous portions, none were found. In the last-mentioned parts of the bed there are numerous obscure vegetable remains, especially leaves of *Cordaites*, leaflets of *Sphenopteris*, and *Trigonocarpa*, apparently of the same species (*T. Sigillariæ*) found with Pupæ in the original repository in the erect Sigillariæ. The appearances were such as to confirm the impression, stated in a previous paper, that the land-shells were drifted along with vegetable matter by some quiet stream, and deposited on the muddy bottom of shallow water.

One object in excavating the bed was to ascertain if any other species of land-animal than *Pupa vetusta* could be obtained from it; and in the first instance, the result appeared purely negative, except in the presence of minute fragments of bone, and of what might have been the chitinous integument of insects. On a more

* *Conulus* Fitz., 1833 (=*Trochiscus* Held., 1837, non Sly.; =*Petasia* Beck, 1837;=*Perforatella* Schlüt.). is a subgenus of *Zonites* Montf. (non Leach, Gray), according to Messrs. Adams, 'Genera of Recent Shells,' vol. ii, p. 116, and their follower Chenu, 'Manual de Conch. et de Paléont.,' vol. i, p. 422. Those who do not care to enter into the modern divisions of the land-shells, may quote the species as a Zonites—or even, speaking loosely, as a Helix.

† Quart. Journ. Geol. Soc., Feb. 1862, and May 1866, p. 121.

‡ Incorrectly stated as 12 feet in the paper last mentioned.

careful examination of the large quantity of fragments of *Pupa* obtained, I was able to select a few small specimens, all of them more or less crushed, which seemed to differ materially from the young of *Pupa vetusta* in form and surface-marking. On submitting these to Dr. Philip P. Carpenter he at once recognized their distinctness from *Pupa vetusta.* Dr. Carpenter names the species *Zonites* (*Conulus*) *priscus* Cpr.

After the description of it he adds "It is probable that there is a third species among the fragments which have been found, presenting a shape more resembling the *H. conulus*, and other trochiform snails. It would be premature however, to venture on a description until more perfect specimens have been obtained."

6. *Notes on Hetch-Hetchy Valley;* by C. F. Hoffmann, (Proc. Ac. Sci. Cal., iii, 368.)—Tuolumne Valley, or Hetch-Hetchy, as it is called by the Indians (the meaning of this word I was unable to ascertain) is situated on Tuolumne river about fifteen miles in a straight line below Tuolumne Meadows and Soda Springs, and about twelve miles north of Yosemite Valley. Its elevation above the sea is from 3,800 to 3,900 feet, a little less than that of Yosemite. The valley is three miles long running nearly east and west, with but little fall in this distance. Near its center it is cut in two by a low spur of shelving granite coming from the south. The lower part forms a large open meadow with excellent grass, one mile in length, and gradually increasing from ten chains to a little over half a mile in width, and only timbered along the edges. The lower part of this meadow terminates in a very narrow cañon, the hills sloping down to the river at an angle of from 40° to 60°, only leaving a channel from six to ten feet wide; the river in the valley having an average width of about fifty feet. This is the principal cause of the overflow in spring time of the lower part of the valley, and probably also has given rise to the report of there being a large lake in the valley. Below this cañon is another small meadow, with a pond. The upper part of Hetch-Hetchy, east of the granite spur, forms a meadow one and three-fourths miles in length, varying from ten to thirty chains in width, well timbered and affording good grazing. The scenery resembles very much that of the Yosemite, although the bluffs are not as high, nor do they extend as far. On the north side of the valley, opposite the granite spur we first have a perpendicular bluff, the top of which is 1,800 feet above the valley; the talus at the base is about five hundred feet above the valley, leaving a precipice of about 1,300 feet. In the spring when the snows are melting a large creek precipitates itself over the western part of this bluff. I was told that this fall is one of the grandest features of the valley, sending its spray all over its lower portion. It was dry, however, at the time of my visit. The fall is 1,000 feet perpendicular, after which it strikes the debris and loses itself among the rocks. About thirty chains farther east we come to the Hetch-Hetchy fall; its height above the valley is 1,700 feet. This fall is not perpendicular, although it appears so from the front, as may be seen from the

photograph by Mr. Harris. It falls in a series of cascades at an angle of about 70°. At the time of my visit the volume of water was much greater than that of Yosemite fall, and I was told that in the spring its roarings can be heard for miles.

Still farther east we have two peaks, shaped very much like "The Three Brothers," in the Yosemite. Their base forms a large, naked and sloping granite wall on the north side of the valley, broken by two timbered shelves, which run horizontally the whole length of the wall. Up to the lower shelf or bend, about eight hundred feet high, the wall, which slopes at an angle of from 45° to 70°. is polished by glaciers, and probably these markings extend still higher up, as on entering the valley the trail followed back of and along a moraine for several miles, the height of which was about 1,200 feet above the valley. The same polish shows itself in places all along the bluffs on both sides, and particularly fine on the granite spur crossing the valley. There is no doubt that the largest branch of the great glacier which originated near Mt. Dana and Mount Lyell, made its way by Soda Springs to this valley. A singular feature of this valley is the total absence of talus or debris at the base of the bluffs, excepting at one place in front of the falls. Another remarkable rock, corresponding with Cathedral Rock in the Yosemite, stands on the south side of the valley, directly opposite Hetch-Hetchy fall; its height is 2,270 feet above the valley. The photograph by Mr. Harris will give some idea of this rock.

7. *Acadian Geology. The Geological structure, Organic remains and Mineral resources of Nova Scotia, New Brunswick, and Prince Edward Island;* by JOHN WILLIAM DAWSON, LL.D., F.R.S., etc. 2nd ed., revised and enlarged. 694 pp. 8vo, with a geological map and numerous illustrations. London, 1868, (Macmillan & Co.; Dawson Bros., Montreal.)—Dr. Dawson's Acadian Geology was originally written largely from his own researches; and the numerous additions to this second edition, are to a considerable extent from his more recent labors, although he acknowledges his indebtedness to various other sources. While he has made the rocks and coal seams of the great coal formation of Nova Scotia his special study, and devotes a large part of his volume to this subject, he has brought out much that is new with regard to all the various rock formations, from the most ancient to those of the human era. The following subjects are among those of general interest discussed in the work:

(1.) The Pre-historic Human period in Acadia, in comparison with that of Europe; (2.) The character and origin of the Boulder Clay and Surface Glaciation, in connection with prevailing theories on these subjects; (3.) The Flora of the Carboniferous period, more especially with reference to the affinities of the several genera of plants, and their relative importance in relation to the formation of Coal—a subject which will be found more fully illustrated in this work than in any previous publication; (4.) The still more curious and ancient Devonian Flora as displayed in New Bruns-

wick; (5.) The Land Animals of the Carboniferous and Devonian periods, of which Acadia has afforded so many examples; (6.) The peculiarities in the nature and age of the Auriferous Deposits of Nova Scotia; (7.) The remarkable Primordial Fauna of Southern New Brunswick, and the peculiar development of the Lower Silurian in the eastern slope of North America.

The work, while full of instruction, is therefore a treatise on many of the deeper principles of the science. The student of American Geology should study it for the new light it throws on the history of the rocks and life of the American Continent; and all interested in geological discussions will find it valuable reading. Dr. Dawson's investigations are made with thoroughness and caution, and hence his conclusions always merit consideration.

The work, besides containing a colored geological map of the region, is illustrated with numerous figures of fossils; among them several of new carboniferous reptiles (their bones and foot-prints), insects, and air breathing mollusks; a large variety of coal plants, both of the Devonian and Carboniferous, many of them novel forms, besides various species illustrating the Post-pliocene era and the life of other periods.

8. *On Subaërial Denudation, and on Cliffs and Escarpments of the Chalk and the Lower Tertiary Beds*, by WM. WHITAKER, F.G.S. 28 pp. 8vo. (From the Geological Magazine, iv, 447, 483, 1867.)—The author concludes his important paper as follows: All geologists know that rivers have made great deposits, as for instance the Wealden Beds, and therefore I do not see how they can avoid allowing that rivers, etc., have been the agents in effecting a great amount of denudation. The solid matter of the Wealden Beds must have existed somewhere before, and must have been worn away by subaërial actions and carried off by streams (the sea being quite out of the question): more too must have been worn away than was deposited afterward by the rivers, for much would be carried out to sea to form a marine deposit. Of course fresh-water beds are both less common and thinner than marine beds, but so also, as aforesaid, the comparatively trifling denudation that has formed our hills and valleys is of far less amount than that which has planed down vast tracts of country and carried off therefrom a great thickness of rock. Perhaps, indeed, the proportion that the effects of marine denudation bear to those of subaërial denudation is not far from the same as that which marine deposits bear to fresh-water deposits.

To those who say that subaërial agents are too small and too weak for the work which has been put to their credit, it may be answered that unlimited time would get over that difficulty. And it should also be borne in mind that good evidence has been brought forward that in late geological times our climate was far more severe than now, and that there may have been a far more rainy period before the present order of things was established; or in other words, that the agents in question were far more powerful than they now are in these islands. Great change indeed has

taken place in historic times; the felling of forests, the draining of land, the embanking and canalization of rivers, the reclaiming of marshes, and the like human handiworks having had their effect in lessening rainfall and floods, and therefore also the wearing action of surface water.

Lastly it seems to me that the discussion of the question of denudation has been argued on a wrong foundation. Surely if we can explain the facts and appearances we see by actions and operations that can be seen going on at the spot now, we are bound to take such explanation until it can be disproved, or until a better one can be given, and we have no right to call in the aid of other and distant operations, without there is some good sign of their having been once present (thus for instance with regard to many rock-basins now far from glaciers, there are unmistakeable signs of their having once contained ice). As a simple matter of reasoning therefore, apart from all scientific truth, we are bound to accept the theory of subaërial denudation until it can be put aside. Geologists should not call on those who hold it, and who show its agreement with things seen, to disprove other theories; but rather should expect its adversaries to disprove it, and to show first, that rain, rivers, ice, springs, damp, and frost are powerless to wear away rocks and to cut out escarpments, valleys and rock-basins, and secondly, that the sea can do and does such work. This, no light task truly, must be done, if it can be done, not by mere assertions of individual opinion, or mere statements based on hasty and prejudiced observations, but by hard work and sound reasoning. Not with us, but with our opponents, lies the *onus probandi.*

9. *Death of Fishes on the coast of the Bay of Fundy;* by Dr. A. Leith Adams, F.G.S., 22nd Regiment.—On the 24th of September, during a heavy gale from the west, impinging almost straight on to the entrance of the Lagoon known as Anderson's cove, enormous numbers of fish were observed floating dead upon the surface of the water, and thrown up in quantities by the waves. On the gale subsiding, the whole surface of the lagoon and its banks were covered with dead fish, to the depth of a foot in some places. It was evident that the shoal had been literally ground to pieces against the rocks by the force of the waves. In conclusion, the author referred to the vast quantities of fossil fish found in the Devonian and other strata, which suggested catastrophes allied to the above incident.—*Proc. Geol. Soc.*, Phil. Mag., July, 1868.

10. *Geological chart of Southern Norway, representing the dioceses of Christiania, Hamar, and Christiansand,* made during the years 1858 to 1865, by MM. Theodore Kjerulf and Tellef Dahll, under the order of the Minister of the interior of the Royal Government of Norway.—This large and beautiful chart is executed in the very best style, in all respects. It is on a scale of 1 : 400,000. As the rocks are mainly granitic and the related metamorphic, the map has a special interest to New England and Canadian Geologists, and those of other regions where this class of rocks prevails.

11. *Revue de Géologie pour les années* 1865 *et* 1866, par M. DELESSE et M. de LAPPARENT. Vol. V. Paris, 1868. (Dunod).—Another of the valuable Annual Reports on the progress of Geology, by Delesse and Lapparent.

III. BOTANY AND ZOOLOGY.

1. *The Book of Evergreens, a practical Treatise on the Coniferæ or Cone-bearing Plants*, by JOSIAH HOOPES. Illustrated. New York. Orange Judd & Co. pp. 435, 12mo.—This book is much wanted, is creditably executed, and is likely to be widely popular. The illustrations are well chosen, numerous, and fairly good. Great pains have been taken to mention, if not to describe all the Conifers known to be anywhere in cultivation, even the tender ones which can only be kept in conservatories; and all the finer species of more favored climes which promise any chance of succeeding in this country are favorably introduced to notice, in the hope that they may somewhere in our broad land secure establishment. The trees are here treated of *con amore*, by one who not only loves them, but knows them well, in a practical and popular way.

After such general commendation, the true work of the critic might begin, and various things be pointed out, typographical, botanical, and literary, which may suggest amendment in future editions. But many of the points to be noticed have probably caught the author's eye before this. For instance, that if he must needs refer *Sequoia*, the genus of our famous Redwoods of California, to the Pine Family proper, or *Abietineæ*, he should at least modify the character of the group so as to receive it. We were at a loss to know why a genus so entirely Cupressineous as *Sequoia* should ever have been referred to *Abietineæ;* but we find that Endlicher was misled by Sir Wm. Hooker's figure of some foliage which he guessed might belong to the great tree of which Douglas and Coulter had spoken, though it is now evident that the trees they saw were of the common Redwood (*S. sempervirens*), and the branches Hooker figured, belonged to a Fir. So Endlicher, who in his latter days only compiled, mixing up in his new genus a Redwood with a Fir, attributed scaly buds to it, and referred the mixture to the *Abietineæ*; and so in his turn misled our present author, who, although aware that Endlicher's *Sequoia gigantea* is a nonentity and a blunder, did not work his way clear of the consequences. At length, when Endlicher's genus really got its second species, and Torrey and Decaine, independently at about the same time, identified it, they only followed the rule of nomenclature in transferring Lindley's specific name of *gigantea* from *Wellingtonia* to *Sequoia*, the homonym of Endlicher, taken from Hooker, having been obliterated in the correction of the error.

We must complain of Mr. Hoopes, that in his compilation of the popular account of this famous tree, he cites too much from the fanciful narrative of Bayard Taylor, and too little from the faithful account of Dr. J. M. Bigelow, and still more for his iteration of

an unwarranted statement that, "according to the annual rings, the age of this tree was 3,100 years"; and this in the very face of Dr. Bigelow's statement before him, of two actual countings of the layers, reducing this age more than half. If further evidence be required, we would refer to the account of a third and very precise measurement and counting, by Mr. De la Rue, which is referred to in this Journal for July, 1866. Notorious as is the incorrectness of the long-current popular statement, it seems impossible to arrest it. In Prof. Brewer's account of the Mariposa Grove, printed by Sir William Hooker, the name was accidentally given as "Maipula"; and so it reads in Mr. Hoopes' extract. Since Mr. Meehan is thanked for constant aid rendered, we could wish that he had informed our author, that *Pinus pungens* inhabits his own state of Pennsylvania as far north as Reading. "Nubiginean Podocarpus," as the English of *P. nubigena*, may set some readers in search of a country whence the species comes, and which they may rightly conclude is *in nubibus*. Imperfections and little mistakes are unavoidable in a first essay even of an experienced writer; but good success at length rewards faithful endeavors. A. G.

2. *The Miscellaneous Botanical Works of* Robert Brown, [edited by J. J. Bennett, for the Ray Society], vol. ii, containing 3, Systematic Memoirs, and 4, Contributions to Systematic Works. 1847. 8vo, pp. 786.—Referring to our former notice of the first volume for some account of this excellent undertaking, we may here barely state that the systematic memoirs, here given, begin with that on *Protaceæ*, followed by the memoir on *Asclepiadeæ* and the *Apocineæ*, published in 1811 by the Wernerian Society of Edinburgh, copies of which are very rare, and end with the paper on *Limnantheæ*, 1833.

In the part entitled contributions to scientific works, we have all that Mr. Brown wrote for the later volumes of the *Hortus Kewensis*, for Richardson's Botanical Appendix, Wallich's *Plantæ Asiaticæ Rariores*, Bennett's *Pl. Javanicæ Rariores*, with extracts from *Bot. Mag.*, *Bot. Reg.*, etc., etc. The whole concludes with a capital Index of Subjects to vol. 2, by the Editor, and an Index of Names of Plants to both volumes, by Mr. Carruthers. A. G.

3. *The Journal of the Linnean Society, Botany*, numbers 42 and 43 just received, interest us chiefly in the paper entitled *Notes on Myrtaceæ*, by George Bentham, the President of the Society.—We have here an elucidation of the principles according to which this family was revised in the new *Genera Plantarum*, and the Australian species worked up in the *Flora Australica*, along with some recent corrections. Among the latter is the announcement that *Nelitris* of Gættner is not the Myrtaceous genus it was supposed to be, (viz.=*Decaspermum* Forster), but the Rubiaceous *Timonius*, as ascertained by Thwaites several years ago. In view of the unwillingness of the authors of the new Genera Plantarum to recognize genera proposed by the predecessors and contemporaries of Linnæus, it may not be superfluous to claim that the now established name of *Timonius* ought not to be superseded simply be-

cause Linnæus happened to overlook the genus altogether. That was the fault of Linnæus, not of Rumphius.

We read with much satisfaction the convincing arguments by which Mr. Bentham vindicates the orthodox view of the calyx-tube of the Rose, of various *Myrtaceæ* and the like, as against the notion lately threatening to prevail, that the consolidated lower part is a hollow receptacle or axis. Finally, as a general term for leaves and their homologues is wanted, one which may play its part in botanical description equally in Latin or any modern vernacular (which "leaf organs," suggested by Mr. Bentham, will hardly do), we propose the word *phyllon*, plural *phylla*, as every way convenient and serviceable,—in this following the analogy of *phyton* already appropriated for the integer of the axis *plus* the *phyllon* or *phylla* it bears.

At this moment we receive Nos. 44 to 47 inclusive. The most interesting and largest papers are those of Mr. Berkeley and Dr. Curtis, which, under the title of *Fungi Cubenses*, describe Mr. Charles Wright's matchless collection of *Fungi* of that island, consisting of 888 species, of which about 66 per cent are peculiar to Cuba, and the greater part new. Also Mr. Darwin's paper on the Hybrid-like nature of the offspring from the illegitimate unions of dimorphic and trimorphic plants; and on the specific differences between the primrose and the cowslip, with proof of the hybrid nature of the common oxlip. A. G.

4. *Théorie de la Feuille*, par M. Casimir De Candolle.—A short, very interesting article in the *Archives des Sciences* of the *Bibliothèque Universelle* of Geneva for May, 1868, and separately issued. A new study of the development and arrangement of the vascular bundles or woody system of the leaf, with a view to a better comprehension of the morphology of the organ. The author concludes that, as a matter of fact, the vascular bundles are arranged on the same general plan in leaf and axis, and theoretically that the leaf is a branch with apex atrophied, thus stopped from continued extension, and with posterior (upper) side atrophied, as shown in the ordinary deficiency of vascular bundles in that region, but that many leaves exhibit a strong tendency to complete the development of that side. All this will not be generally comprehended without some details, which we cannot now enter into. We should prefer to formulate these ideas somewhat differently, and identify the leaf, not with a branch or secondary axis, nor properly with axis at all, but to regard it as the expanded termination, or free portion, of each integer of axis (internode), *i. e.*, the free part of a *phyton*.

The provision in the vascular systems for very various secondary developments, posterior or facial as well as lateral, is interestingly worked out and applied to deduplication or chorisis, which thus gets new support from organogeny; and the application of investigation to the cup of the Rose and the stamen and carpel of Magnolia is not less interesting. A. G.

5. *Obituary announcement.*—It should here be recorded that two veteran botanists have recently deceased in Great Britain, both of whom had many friends in this country to deplore the loss; viz.: NATHANIEL B. WARD, of Clapham Rise, near London, the inventor of the *Ward Case*, and Prof. GEORGE A. WALKER ARNOTT of Glasgow, formerly much associated in botanical work with the late Sir Wm. Hooker, and of late very high authority upon *Diatomaceæ*. Being unable at present to prepare fitting biographical notices, we hope to do so at the close of the current year. A. G.

6. *Note on the Polymorphism of the Anthozoa, and the Structure of the Tubiporæ;* by A. KÖLLIKER.—The polymorphism of individuals, so remarkable among the Acalephæ, has had nothing corresponding to it among the other Cœlenterata; it is therefore a very unexpected discovery that M. Kölliker has lately made, of a true polymorphism in various genera of Anthozoa Alcyonaria. This polymorphism consists in the existence, besides the large individuals capable of taking nourishment and furnished with generative organs, of other, smaller, asexual individuals, which appear essentially to preside over the introduction of sea-water into the organism, and then over its expulsion, and which are perhaps at the same time the seat of an excrementitial secretion. Like the others, these asexual individuals possess a body-cavity divided into chambers by eight septa, and a pyriform stomach with two orifices. On the other hand they are entirely destitute of tentacles; and instead of the eight ordinary mesenteric filaments there are only two, supported upon two consecutive septa. The cavity of the body of these individuals is always in communication with that of the sexual individuals; but the mode in which this communication is effected is liable to vary with the genera.

We may distinguish two types in the mode of distribution of the asexual individuals upon the polyparies. In the first they are distributed in great abundance over the whole polypigerous region of the polypary, among the sexual individuals. This is the case in certain Alcyonids which M. Kölliker refers to the genus *Sarcophyton*, and also in *Veretillum*, *Lituaria*, *Cavernularia*, and *Sarcobelemnon*. In the second case the asexual individuals are restricted to certain perfectly definite places, which, however, are variable according to the genera. Thus in certain species of *Pteroeides* they occur on the lower surface of the pennate leaves of the region serving for attachment, in the form of a larger or smaller plate; in other species of the same genus they are also found at the apex of the polypary; in the *Pennatulæ* the varicosities of the trunk correspond to the places where the asexual individuals are seated; *Funiculina quadrangularis* shows them arranged in longitudinal rows between the sexual individuals; lastly, the *Virgulariæ* always present behind each lamella, upon their trunk, a simple transverse row of asexual individuals.

It is probable that all the Pennatulidæ present a similar dimorphism; at least, in *Renilla* we see, between the fully developed polyps, rudimentary bodies which seem to be individuals of a dif-

ferent form. On the other hand, with the sole exception of the genus *Sarcophyton*, M. Kölliker has sought in vain for dimorphism in the Alcyonidæ and Gorgonidæ. It must not be forgotten, however, that there seem to exist some relations between the buds of the sexual and asexual individuals in the polymorphic polyparies; for, in *Veretillum* at least, the asexual individuals seem, under certain conditions, capable of being transformed into sexual individuals.

M. Kölliker has also been able to investigate a polypary of *Tubipora*, still enveloped by the soft parts, obtained from the Fiji archipelago. Notwithstanding the great resemblance of the polyparies of the *Tubiporæ* to those of the Madripores, the author considers that in their whole structure and development these polyps are Alcyonaria which must occupy a place by the side of the genus *Clavularia*. Both the tentacles and the body of the polyps of *Tubipora* contain spicules.—*Würzburger Zeitung*, January 4, 1868; abstract by E. Claparède in *Bibl. Univ.* February 15, 1868, *Bull. Sci.* p. 171.—*From the Ann. Mag. Nat. Hist.*, IV, i, 227.

7. *A Guide to the study of Insects, and a Treatise on those injurious and beneficial to Crops*, for the use of Colleges, Farm-schools, and Agriculturists; by A. S. Packard, Jr., M.D. With upwards of 500 engravings. Salem, 1868. Part I, June; II, July, and other parts to appear for the present, monthly.—A work of real scientific merit, combining an account of the structure of insects, with facts of practical and popular interest, and abundantly illusted, by one of the best entomologists of the country.

IV. ASTRONOMY.

1. *Notice of new Planets* (98), (99) *and* (101); by James C. Watson, of the Observatory at Ann Arbor, Michigan. (Communicated for this Journal.)—Planet (98) was discovered by Prof. C. H. F. Peters at Clinton, N. Y., on the 18th of April, in the following place:

Ham. Coll. M. T.	α	δ
April 18, $11^h\ 45^m\ 13^s$	$11^h\ 17^m\ 30^s{\cdot}89$	$-1°\ 38'\ 12''{\cdot}7$

Planet (99) was discovered by M. Borelli at Marseilles on the 28th of May. His observation was:

Marseilles M. T.	α	δ
May 28, $10^h\ 26^m\ 51^s$	$13^h\ 24^m\ 7^s{\cdot}92$	$-9°\ 5'\ 49''{\cdot}1$

The planet was then of the 13th magnitude. The notice of the discovery of (100) by me on July 11th has already been communicated to your Journal. I have yet to add that I discovered Planet (101) on the 15th inst. The following places were obtained with difficulty through passing breaks in the clouds:

Ann Arbor M. T.	(101) α	(101) δ
1868. Aug. 15, $12^h\ 7^m\ 38^s$	$23^h\ 53^m\ 39^s{\cdot}61$	$-0°\ 48'\ 39''{\cdot}2$
15, 14 13 27	23 53 36 ·43	0 48 24 ·7
15, 14 46 54	23 53 35 ·68	0 48 20 ·0
20, 13 13 55	23 50 30 ·99	−0 37 38 ·9

The planet shines like a star of the 10th magnitude.

2. *Discovery of another new Planet by Dr. Peters.*—The Utica Morning Herald contains the announcement, dated Aug. 24th, that Dr. C. H. F. PETERS discovered at the Observatory of Hamilton College, Clinton, N. Y., another new planet on Saturday night, the position of which he verified on Monday morning (the 24th). It is in the constellation of Pisces, and had at 3 o'clock of that morning 18° 38′ of right ascension, and 12° 54′ declination, moving slowly to the east. It is now equal to a star of about the eleventh magnitude.

3. *Elementary Lessons in Astronomy;* by J. NORMAN LOCKYER. 348 pp. 16mo, with numerous illustrations. London. 1868. (Macmillan & Co).—Mr. Lockyer has prepared a very convenient and beautifully illustrated work for instruction in the elements of astronomy. The arrangement is different from usual, as the volume opens with the fixed stars and the general system of the universe, and afterwards takes up the special case of the solar system; and in this it has some marked advantages. After chapters on the stars, sun, solar system, it takes up the apparent movements of the heavenly bodies; measurements of time, etc.; telescopic and spectral observations; determination of the apparent places of the heavenly bodies, and their real distances and dimensions; universal gravitation. The illustrations are in superior style. The frontispiece contains the colored spectra of the sun, stars and nebulæ. The beautiful steel plates, one of them a view of a lunar volcano, were furnished the author by W. de la Rue.

4. *Tables for the mutual conversion of Solar and Sidereal time*; by EDWARD LANG, F.R.S.E. 326 pp. 12mo. Edinburgh, 1868. (Wm. Blackwood & Sons).—These tables will prove a great convenience in astronomical computations. The values are given for each tenth second through the day; and precision may thence be readily obtained to the hundredth part of a second. Moreover, tables for the values of each hundredth part up to ten seconds are prefixed, and still further to accommodate the astronomer, the values are given for each solar and sidereal day during the year. The type is large and clear, and the volume is handsomely printed.

V MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Seventeenth Meeting of the American Association for the Advancement of Science, held in Chicago, Illinois, August* 5–12, 1868.—The Chicago meeting of the American Association, under the presidency of B. A. GOULD, was in many respects one of the most satisfactory which has been held. The attendance was large, over two hundred members coming from a distance, while about as many more were added from Chicago and its vicinity. The list of papers entered exceeded one hundred and fifty titles, covering a great variety of subjects in all departments of physical science and Natural History. If few new or startling discoveries were announced, the evidence offered of zealous and successful labor in various fields of research was most satisfactory. The spirit of the

occasion was that of work and an earnest purpose to advance science. But little vague or unmeaning discussion was tolerated or offered.

A number of papers were entered touching more or less directly the question of the antiquity of the human race. Of these by far the most important was that of Prof. J. D. Whitney on the fossil human skull of Calaveras county, California. The evidence as to this alleged discovery has already been given substantially in this Journal, II, vol. xliii, p. 265, and on this point nothing of importance was added. The skull was found it is alleged, (on the evidence of the miner who owns the 'claim' and who says he himself threw it out,) in a shaft sunk in the auriferous gravel of Bald Mountain, not far from Angels Camp in Calaveras Co., Cal. in Sept. 1866, at a depth of about 130 feet and beneath several beds of volcanic matter interstratified with auriferous gold. Its true character was not recognized at the time it was thrown out, a nearly shapeless mass from the agglutination of calcareous matter and gravel impacted especially at the base of the skull. As soon as its true nature was seen it became the object of attention, and was, by Mr. Matteson its discoverer, placed with Dr. Jones of Murphy's, who is well known in that district as a zealous collector of all things rare and curious. This collector soon communicated it to the office of the Geological Survey. This story has been reported to several persons by the original discoverer with essentially the same details. The shaft meantime was filled with water almost immediately after the skull was thrown out, and has never since been pumped out or exhumed, and stands to-day as it was at the date of the alleged discovery.

The important point in Prof. Whitney's communication of these facts at Chicago was the statement of the results obtained by Dr. Jeffries Wyman, of Cambridge in the critical examination of this skull. On clearing away the mass of calcareous tufa which filled the cavity of the zygomatic arch, there were taken out two metatarsal bones, the lower end of a left fibula, part of the ulna end of a sternum bone, all of which may have belonged to the same skeleton with the skull; also a fragment of a human tibia too small for this skeleton, and a shell of *Helix mormonensis.** There was also associated with them a piece of shell fashioned to a flat disc and bored at the center, and a fragment of charcoal.

The gravel was also cleared away from the base of the skull, and the lower jaw isolated and cleaned. The skull had been fractured by violence, with the loss of the left frontal and posterior portion. The teeth and alveolar processes indicated it to belong to an old person. The frontal region was large; but whether the skull was long or broad is not certain. The orbits of the eyes were unsymmetrical, due, as was believed, to the absorption of the alveolar processes of the left jaw producing a depression on that side. These are, substantially, the main points of the case as it now stands. The geological horizon to which it is referable, should it

* *H. Mormonensis* is a species of Helix now living in Calaveras county.

turn out, as the authors believe it will, that the skull was a genuine "find" in the place alleged, is stated by Whitney to be Pliocene or Post-pliocene.

In this connection Prof. Silliman exhibited to the Association one of four molar teeth of Mastodon found in the same, or nearly the same, geological horizon as the alleged Calaveras skull. These teeth, of the origin and position of which there is no doubt, are the same mentioned in an article in this Journal by Prof. S. for May last (vol. xlv, p. 378.) They carry the Mastodon down to a lower horizon than has before been assigned to this mammal in California. It has been the belief hitherto that the great catastrophe of the volcanic outpourings which buried the Table mountains of Calaveras and Tuolumne counties in California had extinguished all the preëxisting races, and that the Mastodon had never been certainly discovered below that horizon. This view is no longer tenable, and the Mastodon is here conclusively shown to reach quite to the base of the deep-lying gold placers; and if the Calaveras skull stands the test of subsequent investigation man was his companion in these early days.

Prof. W. P. Blake, in the same connection, exhibited drawings of stone implements, and stated facts of their alleged coëxistence with fragments of human crania in the deep-lying gold placers. All Prof. Blake's statements were based on specimens and information obtained from Dr. Snell, of Sonora in California, who imparted them to Prof. B., who had not in person obtained them. Here again we lack the evidence so essential to establish unquestionable authenticity.

Among the evening proceedings should be specially noticed the Address of Pres. F. A. P. Barnard, the presiding officer at the Buffalo meeting, which, owing to the absence of the author in Paris last year, was not delivered at Burlington in due course, and was consequently given on the evening of the opening at Chicago. It was characterized by that encyclopedic comprehensiveness and interest which have rendered Dr. Barnard's addresses so favorably known.

The eulogy on Alexander Dallas Bache, by the President, Dr. B. A. Gould, was delivered on the evening of Aug. 6th, at Library Hall, and was worthy of the reputation of its author. It was a noble tribute to the life and services of the lamented scientist, who, more than any man of his time in the United States, combined with wonderful administrative ability the highest scientific attainments, and whose name and memory are cherished by all who cultivate science, whether in America or abroad.

To the labor of the Local Committee at Chicago, the Association is indebted for the enjoyment of an occasion which will be long remembered with delight by all those had the privilege of partaking of the all but universal hospitalities, which left no moment of the day or evening unemployed by the most agreeable social and intellectual pleasures. Not content with providing for the personal comfort and entertainment of their numerous guests while within

the walls of this wonderful city of the northwest, the Committee arranged for a number of excursions, after the adjournment, to various, and even very distant points of scientific interest,—to the coal-fields of Illinois—the lead mines of Galena and Dubuque—the copper regions of Lake Superior, and, more than all, to the Rocky mountains and the present terminus of the great Pacific Railroad, not less than 1,250 miles from Chicago, on the path to San Francisco. Return tickets were also provided for all who had to pass over most of the long routes of 1,000 miles to the Atlantic. Surely the members of the American Association for the Advancement of Science, and their ladies who were so fortunate as to accompany them, have good occasion to remember with pleasure the more than princely entertainment and magnificent hospitalities of Chicago.

The next meeting of the Association will be held in the city of Salem, Massachusetts, commencing August 18, 1869, under the Presidency of Col. J. W. Foster, of Chicago. B. S.

The following is a list of the papers read at the meeting:

1. In General Session.

1. On the Application of Electricity to the Maintenance of the Vibrations of the Tuning-fork; and of the Tuning-fork to the Excitement of Vibrations in Cords and Threads; Joseph Lovering.
2. Steam Boilers and the various Causes assigned for their Explosions, illustrated by Facts, Drawings and Experiments; Joseph A. Miller.
3. On the Artistic Evidence of the Remote Colonization of the North-Western or American Continent by Maritime People of Distinct Nationalities before the Modern Era; J. H. Gibbon.
4. Vestiges of Pre-historic Races in California; W. P. Blake.
5. The Antiquity of Man in North America; J. W. Foster.
6. The Fossil Human Skull of Calaveras County, California; J. D. Whitney.

2. In Section A.

Mathematics, Physics, and Chemistry.

1. On the Theory of Luminous Hydrocarbon Flames; Eugene W. Hilgard.
2. The Statics of the Four Types of Modern Chemistry, with Special Regard to the Water Type $\left.\begin{matrix}H\\H\end{matrix}\right\}O$; Gustavus Hinrichs.
3. A New and General Law, determining the Atomic Volume and Boiling Point of a great number of Carbon Compounds; Gustavus Hinrichs.
4. The Calculation of the Crystalline Form of the Anhydrous Carbonates, Nitrates, Sulphates, Perchlorates, Permanganates, and other Salts of the Composition AB_3C, or AB_4C; Gustavus Hinrichs.
5. A New Physio-Geographical Explanation of the Tides, etc.; Theodore C. Hilgard.
6. Some Experiments on the Influence of the Physical Condition upon the Personal Equation, in Transit Observations; W. A. Rogers.
7. A New Formula for the Reduction of Observations in the Prime Vertical, analogous to the Formula for the Reduction of Meridian Observations; W. A. Rogers.
8. On the Chemico-Geological Relations of the Metals; T. S. Hunt.
9. Influence of the Moon upon the Weather; Elias Loomis.
10. The Recent Contributions of Science to the Arts of Dyeing and Printing Woollen Tissues; John L. Hayes.
11. Meteorolites from Mexico and Poland; Louis Feuchtwanger.

12. On the Combining Power of the Chemical Elements; S. D. TILLMAN.
13. Further Notice of Experiments on Snow and Ice at a Temperature below 32° F.; EDWARD HUNGERFORD.
14. On Hansen's Theory of the Physical Constitution of the Moon; SIMON NEWCOMB.
15. Resuscitation of the Cincinnati Observatory; CLEVELAND ABBE.
16. The Source of Free Hydrochloric Acid in the Gastric Juice; E. N. HORSFORD.
17. Relations of the Metamorphoses of the Phosphates to Waste and Repair; E. N. HORSFORD.
18. Economy in the Conversion of Beef into Food; E. N. HORSFORD.
19. Phosphoric Acid a Constituent of Butter; E. N. HORSFORD.
20. Source of Muscular Power; E. N. HORSFORD.
21. Fluorine a Constituent of the Brain; E. N. HORSFORD.
22. The Hot Term of July, 1868; O. N. STODDARD.
23. The Principles of Statistics as applied to the Census; F. B. HOUGH.
24. Remarks on the Galvanic Battery; G. W. HOUGH.
25. On a proposed new Mechanism for the study of Galvanic Batteries; G. W. HOUGH.
26. Remarks on the total Disturbance of the Barometrical Column; G. W. HOUGH.
27. On the Mathematical Investigations made for the Construction of the Illinois and St. Louis Bridge; WILLIAM CHAUVENET.
28. On a Method of Measuring very Small Rectilinear Motions; WILLIAM CHAUVENET.
29. On the Laws of Ocean Currents; J. S. GRIMES.
30. Hough's Barometrograph as applied to the Investigation of the Storm Curve; J. H. COFFIN.
31. The Profiles of Blast Furnaces; THOMAS EGLESTON.
32. The Nature of Electric Discharge; O. N. STODDARD.
33. On the Evaporative Power of the Sun near the Base of the Sierra Nevada, in Calaveras County, California; BENJAMIN SILLIMAN.
34. On the Relation Between the Atomic Value of Different Metals, and their Paramagnetic and Diamagnetic Properties; P. H. VANDER WEYDE.
35. On the Molecular Arrangement of the Inorganic Acids; GEORGE F. BARKER.
36. Theory of the Prediction of Star Places; T. H. SAFFORD.
37. Remarks on the Secular Variations of the Eccentricities and Perihelia of the Eight Principal Planets; J. N. STOCKWELL.
38. Description and Application of the Heliostat, and Method of Running True Meridian Lines in Surveying; MICHAEL W. MCDERMOTT.
39. Tides in Lakes; H. A. NEWTON.
40. On the Tides of Lake Michigan; W. FERREL.
41. Notes on the Defects of Lightning Rods; JAMES BUSHEE.
42. Some New Facts and Views concerning Aluminum; HENRY WURTZ.
43. The Application of Carbonic Acid Gas, in the Extinguishment of Fire; E. L. BUTTRICK.
44. Atomic Motion; H. F. WALLING.
45. The Higher Law of Correlation; O. N. STODDARD.
46. The Periodic Law in the Failure of Harvests and Inundations, with suggestions as to their Insurance; GEORGE A. LEAKIN.
47. On Methods of Amalgamation in the Treatment of Gold Ores; BENJAMIN SILLIMAN.
48. On Redemption Periods of Monetary Values which involve Life Contingencies; E. B. ELLIOTT.
49. On the Metrical Unification of International Coinage; E. B. ELLIOTT.
50. Exhibition of a New Selenograph; JEROME ALLEN.
51. On a New Method of Measurement Map Drawing for Schools; JEROME ALLEN.
52. Note on Epitrochoidal Teeth; WILLIAM WATSON.

3. In Section B.

Geology and Natural History.

1. On the Genus of extinct Sea-Saurians, *Elasmosaurus;* Edward D. Cope.
2. Phases of Glacial Action in Maine at the Close of the Drift Period; N. T. True.
3. Bibliography of Entomology in the United States and Canada, since 1862; John G. Morris.
4. On the Leaves of Coniferous Plants; Thomas Meehan.
5. On the Geology of the Mississippi Delta, and the Salt Deposit of Petite Anse; Eugene W. Hilgard.
6. Effects of Atmospheric Changes on the Eruptions of the great Geyser of Iceland; P. A. Chadbourne.
7. On the Boulder Field in Cedar County, Iowa; Rush Emery.
8. On the Upper Silurian and Devonian Rocks of Ontario; T. S. Hunt.
9. On Gold in the Laurentian Rocks of Canada; T. S. Hunt.
10. On the Gold Region of Nova Scotia; T. S. Hunt.
11. Origin of Prairies; J. W. Foster.
12. Exhibition of the Crania of Bootherium and Castoroides, with Remarks on their Geological Position and their Living Analogues; J. W. Foster.
13. On the Stratigraphical Relations of the Fossil Horse in the United States; Charles Whittlesey.
14. Brief Remarks on the Botany, Meteorology, and Geology of Mount Mansfield, Vermont; James Hyatt.
15. The Habitable Features of the North American Continental Plateau near the Line of 35° Parallel North Latitude; containing a General Summary of Conclusions derived from a Review of its Aboriginal Population and Natural Features; C. C. Parry.
16. Description of a New Species of *Protichnites* from the Potsdam Sandstone of New York; O. C. Marsh.
17. Notice of some new Vertebrate remains from the Tertiary of New Jersey; O. C. Marsh.
18. On the preservation of color in fossils from Paleozoic formations; O. C. Marsh.
19. The progress and present condition of the Geological Survey of California; J. D. Whitney.
20. The Yosemite Valley; J. D. Whitney.
21. Some points in the Surface Geology of the western side of the American Continent; J. D. Whitney.
22. The Quebec Group in Northern New Hampshire; C. H. Hitchcock.
2 . The supposed Triassic Foot-marks in Kansas; C. H. Hitchcock.
24. On the Geological age and equivalents of the Marshall Group; A. Winchell.
25. Experiment illustrating the Flow of Glaciers; Edward Hungerford
26. On the Plasticity of rocks, and origin of the Structure of the so-called Grave Stone Slates of California; W. P. Blake.
27. On the gradual Desiccation of the western portion of North America; W. P. Blake.
28. On the Physical Geography of the Continent of North America during the different Geological Periods; J. S. Newberry.
29. On two new fossil trees, the oldest known, found by Rev. H. Herzer, in the Devonian rocks of Ohio; J. S. Newberry.
30. On the surface geology of the Basin of the Great Lakes and the Upper Mississippi Valley; J. S. Newberry.
31. Fuel Resources of Illinois; A. H. Worthen.
32. Fossil Fishes, Insects, Crustacea, etc., of the Coal Measures of Grundy Co., Illinois; A. H. Worthen.
33. Geological section of Ohio; E. B. Andrews.
34. Anatomical distinction of vegetable structure rectified, followed by the circuit of generation of fresh water Algæ; T. C. Hilgard.

35. The vertebral type of the cranium a Quinary one; T. C. HILGARD.
36. On *Elasmognathus* and its relations to the Tapiridæ generally; THEODORE GILL.
37. Sketch of the Topography, Geology and Antiquities of the Caucasus; F. VON KOSCHKULL.
38. Superficial geology of the lake shore near Chicago; J. S. JEWELL.
39. On the occurrence of the Mastodon in the deep-lying gold placers of California; BENJAMIN SILLIMAN.
40. On the old lake beds of the Prairie region; S. J. WALLACE.
41. On the formation of shells and Belemnites, and Phosphates of Iron at Mullica Hill, Gloucester County, N. J.; A. B. ENGSTROM.
42. On certain physical features of the Mississippi River; G. K. WARREN.
43. The Darwinian theory of Development; CHARLES MORAN.
44. On the Interpretation of Fossils; B. WATERHOUSE HAWKINS.

The titles of 49 other papers were handed to the Standing Committee, but for want of time, a few of them only were read by title, and most of them not at all. This makes the whole number of papers presented at the meeting, 151.

2. *On the fall of Rain, as affected by the Moon;** by PLINY EARLE CHASE.—The discussion of the Moon's influence on the weather has been recently revived by European meteorologists, and an article by George Dines, Esq., in the Proceedings of the Meteorological Society, No. 36, contains some valuable tables, which seem to me worthy of special attention. Mr. Dines was entrusted with the Journal of Miss Caroline Molesworth, of Cobham Lodge, Surrey, from which he was "enabled to extract the rain which fell during each day of the moon's age, for a period extending (with few interruptions) over forty years." From these extracts he has condensed synopses for five years, ten years, twenty years, and forty years, arranged according to the several days of the moon's age, with two subordinate tables, one of which contains "groups of three or four days preceding and following each change of the moon," the other gives the proportionate amounts of rain in the successive quarters of the lunar month. There are also statements of the number of days on which ·01 inches or more of rain fell, and of the number of days on which ·25 inches or more fell, during the entire period. These summaries, as well as the data on which they are based, lead the author to the "decided opinion that the fall of rain *is in no way influenced by the changes of the moon, or by the moon's age.*"

The influence of the moon on the ocean tides, on the fluctuations of the barometer and magnetic needle, and on the winds, (see Glaisher's Tables, Proc. Met. Soc., No. 30), is so well known that the inference of a similar influence on the rainfall seems almost irresistible. I was therefore naturally startled at Mr. Dine's conclusion, and as his evident impartiality forbade any doubts of the accuracy of his results, I was led to examine into the correctness of his method. The observations cover a period of a little more than two Metonic cycles, and by the tabular arrangement the effects of the revolution of the moon's nodes, as well as those of va-

* From the Proceedings of the American Philosophical Society, June 19, 1868.

rying latitude and declination, are almost entirely eliminated.* The station is in a high northern latitude, and within the influence of the Gulf Stream, therefore the lunar modifications of the climate, especially when those modifications are estimated merely from their equatorial means, are comparatively unimportant; the four lunar quarters, each embracing an entire interval between a spring and a neap tide, are precisely the divisions which, when the periods are sufficiently extended, would eliminate most of the remaining evidences of lunar disturbance. Still, if we examine the half-cycles, or ten-year periods, we may observe that the half-months of lunar conjunction present a rainfall alternately greater and less than the half-months of opposition, a fact which suggests a possible dependence on the alternately predominating north and south latitude.

It is impossible, from the tables, to determine the extent of such a dependence. Mr. Dines, who has the necessary materials at his command, may perhaps deem the subject worthy of further investigation. The division of the month may be easily altered so as to correspond more nearly with the times of presumable maximum and minimum disturbance. If we regard the day of each change of phase as the middle day of a week, (counting the half-sum of the 5th and 12th days in the first quarter, and the half-sum of the 20th and 27th days in the third quarter), the seven days' aggregates in Table 1, and in the two summaries on pp. 136–7 (*loc. cit.*), will furnish the following results:

Aggregates of Tabular Ratios and Number of Rainy Days.	Week of New Moon.	Week of First Quar.	Week of Full Moon.	Week of Last Quar.	Two Weeks of Syzygy.	Two weeks of Quadrature.
1825 to 1829,	770	606	679	766	1449	1372
1830 to 1834,	657	797	599	731	1256	1528
1835 to 1839,	598	800	668	617	1266	1417
1840 to 1844,	643	708	760	662	1403	1370
1845 to 1849,	830	726	602	627	1432	1353
1850 to 1854,	687	726	614	810	1301	1536
1855 to 1859,	507	722	728	735	1235	1457
1860 to 1864,	706	680	679	754	1385	1434
1825 to 1834,	716	698	638	750	1354	1448
1835 to 1844,	618	753	771	639	1389	1392
1845 to 1854,	765	724	608	711	1373	1435
1855 to 1864,	651	701	702	744	1353	1445
1825 to 1844,	667	726	704	693	1371	1419
1845 to 1864,	708	714	656	729	1364	1443
1825 to 1864,	687	721	681	709	1368	1430
·01 inch or more,	1490	1484	1446	1511	2936	2995
·25 inches or more,	260	268	250	272	510	540

* Lubbock found a barometric elevation of nearly ·1 inch for 17° increase of declination. *Phil. Trans.*, 1841, p. 73. He appears to have been led to the investigation by Howard's remarks "on a cycle of eighteen years in the mean annual height of the barometer in the climate of London." *Clim. of London*, 2d ed., v. i, p. 172; *Phil. Trans.*, 1841, p. 277, seq. See also Zenger's discussion of the mean annual temperature, as affected by the revolution of the moon's nodes and apsides. *Phil. Mag.*, v, 35, June, 1868.

It may thus be seen that, *notwithstanding the complete veiling of all the disturbances which may be due to the moon's variable distance and declination, there was a marked tendency to increase at quadrature and to decrease at syzygy, both in the amount of rain and in the number of rainy days.* This tendency, which becomes evident even in the majority of the five years' groupings, is uniformly shown in all the groups of ten years, twenty years, and forty years, as well as in the number of rainy days and in the number of heavy rains during the entire period.

In a future communication I propose to discuss the observations at Pennsylvania Hospital, which demonstrate the existence of similar tides at Philadelphia. The forty years' aggregates (1825 to 1864, inclusive, at each station) exhibit the following ratios of weekly rainfall:

	Surrey.	Philadelphia.
Week of new moon,	98·2	97·6
" first quarter,	103·1	100·3
" full moon,	97·4	95·8
" last quarter,	101·4	106·3

The tide is so strongly marked at Mussoorie, on the southern range of the Himalaya Mountains, as to be strikingly shown by 13 years' observations (1854–1866) on the days of change. (See Mr. Hennessey's communication, Proc. Roy. Soc., v, 16, December 12, 1867.) The mean results are,

	Average daily fall.	Ratios.
Day of new moon,	·402	86·2
" first quarter,	·535	114·7
" full moon,	·399	85·6
" last quarter,	·529	113·5

3. *On a supposed connection between the amount of Rainfall and the changes of the Moon*, being an extract of a letter from J. H. N. Hennessey, Esq., First Assistant on the Great Trigonometrical Survey of India, to General Sabine, R.A., Pres. R.S., (Proc. Roy. Soc., xvi, 213.)—Allow me now to say a few words in connection with the inclosed paper. There appears to prevail a belief, more or less popular, to the effect that more rain falls at "the changes of the moon" than on the intermediate days of a lunation. As I happened to possess a record of the rainfall at the office of the Superintendent of the Great Trigonometrical Survey of Mussoorie, extending over thirteen consecutive years, I obtained Colonel Walker's permission to make use of the Register, in connection with this popular belief.

The results tabulated have been obtained by employing an *average daily* fall as the means for comparing the fall *at* "the changes" with that at intermediate intervals. The method of calculation adopted is explained in the foot-note to the Table. The annual average result may be stated thus:—

	Inch.
At "the changes" of the moon the *mean daily* fall of rain is	0·466
Between "the changes" of the moon the mean daily fall is	0·525

which is in opposition to the popular belief on the subject. I inclose the Table, on the chance of its proving sufficiently interesting to be noticed.

Average daily fall of rain between successive quarters and at each quarter of the moon from 1*st of May to* 31*st of October of each year*, measured at the office of the Superintendent of the Great Trigonometrical Survey of India. The office stands in Mussoorie, on the most southern range of the Himalaya Mountains, lat. N. 30° 28′, long. E. of Greenwich 78° 7′; height above mean sea-level 6500 feet.

Year	Average Daily Fall.								Total Fall from May 1 to Oct. 31.
	◐ to ●	●	● to ☽	☽	☽ to ○	○	○ to ◐	◐	
	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inches.
1854....	·644	·374	·813	·176	·630	·096	·512	·621	100·72
1855....	·456	·204	·360	·918	·311	·356	·753	·733	85·85
1856....	·732	·745	·703	·237	·397	·588	·347	·340	93·28
1857....	·280	·319	·794	1·013	·521	·136	·368	·606	88·27
1858....	·402	·448	·485	·298	·518	·157	·705	·373	84 61
1859....	·665	·263	·253	·642	·306	·253	·570	·583	78·31
1860....	·356	·228	·430	·719	·564	·205	·301	·073	65·81
1861....	·685	·678	1·014	·372	1·332	·287	·577	·855	141·16
1862....	·611	·620	·513	·651	·364	·852	·645	·530	93·91
1863....	·848	·342	·862	·932	·511	·595	·291	·546	93·03
1864....	·762	·409	·545	·292	·394	·828	·237	·352	82·19
1865....	·543	·235	·276	·120	·443	·526	·518	·785	76·37
1866....	·135	·360	·402	·580	·686	·809	·452	·483	81·15
Means of columns	·509	·402	·573	·535	·533	·399	·483	·529	89·589

General mean of ● ☽ ○ ◐ .... 0·466 inch.
General mean of ◐ to ●, ● to ☽, ☽ to ○, ○ to ◐ .... 0·525 "

Note.—The rainfall during the *preceding* twenty-four hours was measured daily at mean noon. Suppose m_1, m_2, m_3, m_4, m_5, m_6, m_7, m_8, m_9, to denote nine such consecutive measurements of daily rainfall, registered at Mussoorie mean noon, respectively on the 1st, 2d, . . . 9th of the month, and that the moon entered her first quarter at an hour nearer to noon of the 1st than to the preceding or succeeding noons. In this case the arithmetical mean of m_1 and m_2 has been entered in column ☽ as the average *daily* fall *at* the first quarter. Similarly, if full moon occurred nearest to noon of the 8th, the quantity $\frac{m_8+m_9}{2}$ has been reckoned as the average *daily* fall *at* full moon; and $\frac{m_3+m_4+m_5+m_6+m_7}{5}$ represents the average *daily* fall *from* ☽ to ○. The foregoing Table has been prepared under these conditions by Baboo Dwarkanath Dutt, Computor to the Great Trigonometrical Survey of India.

4. *On a new Meteorite.*—H. B. Geinitz has described a new meteorite, handed to him for examination in September last by Pastor Nürnberger of Nöbdenitz, near Schmölln, duchy of Altenburg, who found it about a foot below the surface of the ground in digging a ditch near his dwelling. It was irregularly hexagonal in shape, and appeared to be a portion of a much larger stone. In its longest diameter it measured 10·5 centimeters, in breadth 9 centimeters, in thickness from 2–5 centimeters. Its surface was

covered with the characteristic black, sometimes dark brown crust of ferric oxyd and hydrate, with here and there traces of tile ores and malachite. The entire mass weighed 1·2194 kilograms; with the greatest difficulty it was broken into fragments, the largest of which, weighing 976 grams, was, together with some smaller fragments, retained by Pastor N., a second piece, 163·75 grams in weight, was deposited in the Royal Museum in Dresden. The structure of this fragment is compact, it is divided only with difficulty, is slightly malleable, strongly magnetic, and possesses a finely granular fracture, which when fresh, exhibits a light steel-gray color. The hardness is from 5–6; gravity of the interior portion, 7·06, and of fragments with the crust, 6·75 and 5·8 (Fleck).

The analysis was made by Dr. Fleck. After removing a trace of insoluble residue, in which silica was recognized, the fresh interior material afforded:

Iron,	88·125
Copper,	9·013
Nickel,	1·340
Tin,	1·321
Cobalt and Chromium,	traces.
	99·799

—*Ber. Soc. Isis zu Dresden, Nov.* 14, 1867.

OBITUARY.

Professor Matteucci.—The Italian papers record the death of Professor Matteucci on Friday morning last, at Florence, after a short illness. The deceased was an Italian senator and Minister of Public Instruction, in which capacity he was very active in promoting the extension of education. But he was better known as a man of science than as a politician or a minister. He obtained, in 1844, the prizes of the French Academy of Sciences and the Copley Medal of the Royal Society for his investigations in electro-physiology. His "Lectures on Physics" passed through four editions. He published also "A Manual of Telegraphy," "A Treatise on Electro-physiological Phenomena," "Elements of Electricity as applied to the Arts," and "Lectures on the Physico-chemical Phenomena of Living Bodies," which has been translated into English and French.—*Chemical News.*

VI. MISCELLANEOUS BIBLIOGRAPHY.

1. *Cambridge Physics: A Hand-book of Natural Philosophy;* by W. J. Rolfe and J. A. Gillet, Teachers in the High School, Cambridge, Mass. Boston, 1868, (Woolworth, Ainsworth & Co.) —This is a School Text-book, based on the freshest authorities, clear of traditional rubbish, and presenting, as we judge from a cursory examination, more excellencies and fewer defects than any of the school philosophies in use. It consists of two distinct works, or parts, bound together: the first, the elements of Natural Phi-

losophy under the three heads, Pressure, Motion, and Machines and Sources of Mechanical Power; the second, Sound, Light, and Heat, with an appendix on Energy. Electricity and Magnetism are omitted, because embraced in another volume of the same series on chemistry.

Full explanations of difficult points are not to be expected in an elementary work; yet, if attempted at all, they should not, in our view, be so defective and inadequate as is that of the difference between Newton's theoretical and the observed velocity of sound, p. 8, part II.

The book is well furnished with illustrations, problems, questions, and indexes. C. S. L.

2. *Braithwaite's Retrospect of Practical Medicine and Surgery.* Part LVI, January, 1868. New York. (W. A. Townsend & Adams.)

Ranking's Half-Yearly Abstract of the Medical Sciences. Vol. XLVI, from July to December, 1867. Philadelphia. (Henry C. Lea).—These semi-annual digests of the current medical literature of the world, reprinted from the London editions, come to us at irregular intrevals. The former contains a hundred and fourteen articles, the latter, two hundred and sixty eight; presenting in a small compass, and for a small price, an abstract of the most important papers published within the last six months, in the various departments of the medical sciences.

We know of but one valid objection to these publications, which have become so indispensable to the busy practitioner,—their contents are so valuable and complete, and conveniently arranged, that in many cases the temptation to depend entirely upon them, to the exclusion of a wider reading of medical authors, may not be successfully resisted.

3. *Archives du Musée Teyler*, vol. I, Fascicule 1, 2, 3, Harlem, 1866, 1867, 1868. Roy. 8vo.—The Archives of the Teyler Museum are issued by the Directors of the Teyler foundation, and are to appear from time to time as memoirs may be ready for publication. The papers in the three parts thus far issued, are elaborate, and pertain to questions in Physics and Chemistry; the subjects are the following: On the determination of the lengths of the wave of the Solar Spectrum, by M. S. M. Van der Willigen; On the refraction and dispersion of flint glass, by the same; On the determination of the indices of refraction and on the dispersion of mixtures of sulphuric acid and water, by the same; On a case of explosion by illuminating gas, by the same; On the indices of refraction of some saline solutions and two other liquids of feeble dispersion, by the same; On the refraction of water, by the same. The Archives contain also a continuation of the catalogue of the library and of the museum.

4. *The American Naturalist*, Vol. II, No. 5, July. *A Sea-side number.* Salem, Mass.—The midsummer number of this excellent magazine is very appropriately devoted exclusively to marine Zoölogy and Botany, which will make it a very useful and interesting work for those who visit the sea-side summer resorts. It contains,

Sea-Weeds, by J. L. Russell; A Stroll by the Sea-side, by E. S. Morse (illustrated); Our Sea-Anemones, by A. E. Verrill; The Marine Aquarium; A few Sea-Worms, by A. S. Packard (illustrated); Good Books for the Sea-side; Insects living in the Sea, by A. S. Packard (illustrated); Directions for Collecting Marine Animals, etc.

5. *Reliquiæ Aquitanicæ;* being contributions to the Archæology and Palæontology of Perigord and the adjoining Provinces of Southern France, by E. LARTET and H. CHRISTY: edited by Thomas Rupert Jones. Part V. Pages 33–52, and 73–80. Plates A, xv, xviii, and Sketches of the Vezere, Nos. 1 and 2. London, April, 1868. (H. Baillière).—This new part, like the preceding, contains numerous excellent figures of flint arrow heads, chippings, etc., besides a page of cuts representing North American Implements of stone, metal and bone, for comparison with the more ancient implements of Europe.

6. *A treatise on concentration of all kinds of ores, including the chlorination process for gold-bearing sulphurets, arseniurets, and gold and silver ores generally:* by GUIDO KUSTEL, Mining Engineer and Metallurgist, author of "Nevada and California Processes of Silver and Gold Extraction." 260 pp. 8vo, with 120 Diagrams on 7 plates. San Francisco, 1868. (Office of the Mining and Scientific Press.) We defer a notice of this important work to another number.

7. *Catalogue de Minéraux, Roches, Fossiles, Modèles en Platre et Modèles en Bois des cristaux à vendre dans le Comptoir Minéralogique Rhénan,* (à Bonn) du AUGUSTE KRANTZ, Dr. der Sci. ix ed., 1868.—A new edition of Dr. Krantz's Catalogue, showing extensive collections of minerals, rocks, fossils, casts, etc., the largest in fact in the world, and also very reasonable charges.

Proceedings of the American Association for the advancement of Science, 16th meeting, held at Burlington, Vermont, Aug. 1867. 178 pp. 8vo. Cambridge. 1868. Published by Joseph Lovering, Permanent Secretary.

A system of instruction in the practical use of the Blowpipe. 2nd ed., with an appendix and index, by G. W. Plympton, A.M. 288 pp. 12mo. New York: (D. Van Nostrand.)

Discussion of the West India Cyclone of Oct. 29, 30, 1867. Prepared by order of Commodore B. F. Sands, U. S. N., Superintendent U. S. N. Observatory, by J. R. Eastman, Prof. Math., U. S. N. 18 pp. with a map. 8vo, Washington, 1868.

A Dictionary of Chemistry, and the allied branches of other sciences, by Henry Watts, B.A., vol. v, completing the work. London, 1868: (Longmans).

Elemente der Mineralogie von Dr. Carl Friedrich Naumann, Prov. Univ. Leipzig, etc, 7th ed., 1st part with numerous illustrations. Leipzig, (W. Engelman).

Lehrbuch der physikalischen Mineralogie, von Dr. A. Schrauf. vol. 2nd, 426 pp. 8vo, with numerous illustrations. Vienna, 1868: (W. Braumüller).

Atlas of charts of the Meteor tracks contained in the British Association Catalogue of observations of Luminous Meteors, extending over the years 1845 to 1846; showing the principal dates of apparition, the limits of duration, the positions of the radiant points, the distinctive appellations (by Heis and Greg), and the specific appearances of the meteors of all the general and special meteoric showers hitherto observed in the northern hemisphere throughout the year. Prepared for the Luminous Meteor Committee of the British Association by R. P. Greg and A. S. Herschel. 23 plates, folio, 1867.

Report of the Astronomer Royal to the Board of Visitors of the Royal Observatory, Greenwich, June 6, 1868.

Catalogo degli Uranatmi (ossia stille Cadenti) &c.; catalogue of shooting stars observed at Rome in the years 1861–1867 by Madame Caterina Scarpellini. 4° 16 pp. Rome, 1868.

Vierteljahrschrift der Astronomischen Gesellschaft, I and II, Jahrgang, 8°. 1866 and 1868. Leipzig, (W. Engelmann.)

Publicationen der Astronomischen Gesellschaft, I—VI, VIII. 4. 1865—1866, brosch.

I. Hülfstafeln zur Berechnung specieller Störungen, enthaltend die rechtwinkligen Ekliptical-Coordinaten und die vom Orte des gestörten Körpers unabhängigen Theile der störenden Kräfte für die Planeten Venus, Erde, Mars, Jupiter, Saturn, Uranus und Neptun von 1830–1864. 1865.

II. Lesser, Dr. Otto, Tafeln der Metis, mit Berücksichtigung der Störungen durch Jupiter und Saturn. 1865.

III. Weiler, Dr. A., über das Problem der drei Körper in Allgemeinen und ins Besondere in seiner Anwendung auf die Theorie des Mondes. 1866.

IV. Hoüel, Dr. G. J., Tables pour la reduction du temps en parties décimales du jour. 1866.

V. Auwers, Arth., Reduction der Beobachtungen der Fundamentalsterne am Passageinstrument der Sternwarte zu Palermo in den Jahren 1803–1805 und Bestimmung der mittleren Rectascensionen für 1805. 1866.

VI. Rechtwinkelige und Polarcoordinaten des Jupiter (nach Bouvard's Tafeln), sowie Componenten der störenden Kräfte, mit denen Jupiter auf die Sonne wirkt von 1770—1830. 1866.

VIII. Schjellerup, Genäherte Oerter der Fixsterne von welchen in den astronomischen Nachrichten Band 1—66 selbständige Beobachtungen angeführt sind für die Epoche 1855 hergeleitet und nach den geraden Aufsteigungen geordnet. 1867.

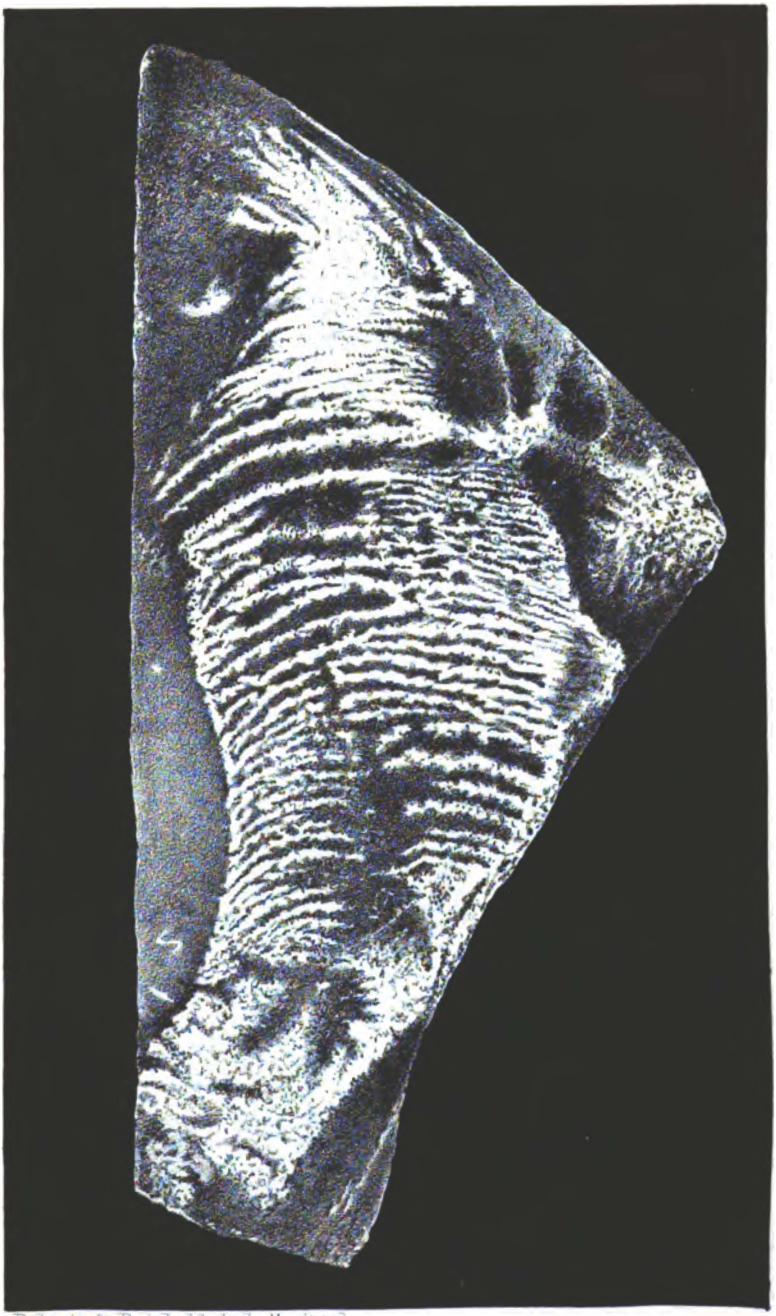
Travaux Publiés et constructions civiles; Rapports du Jury international reunis par ordre de son Excellence M. de Forcade la Roquette, Ministre de l'agriculture, du commerce et des Travaux publics. 458 pp. 8vo. Paris, 1868.

PROCEEDINGS BOSTON SOC. NAT. HIST., vol. xii.—Page 1, Report of the Custodian, additions to the Library and Museum, Trustees accounts. On page 75 the officers for the ensuing year are stated as follows: President, JEFFRIES WYMAN; Vice Presidents, C. T. JACKSON, T. T. BOUVÉ; Corresponding Secretary, SAMUEL L. ABBOT; Recording Secretary, S. H. SCUDDER; Treasurer, EDWARD PICKERING; Librarian and Custodian, S. H. SCUDDER. Page 77, further enumeration of New England Fungi; *C. C. Frost* of Brattleboro, Vt.

TRANS. ACAD. SCI. ST. LOUIS, vol. II.—Page 459, North American Species of Juncus (continued); *Engelmann.*—p. 499, Account of the passage through the great Canon of the Colorado, by Mr. James White, in 1867, with Geological Notes; *Parry.*—p. 504, Age of the Porphyry Hills of southeast Missouri; *Harrison.*—p. 507, Mr. Meek's notes on his preliminary Report of the Geology of Kansas; *Swallow.*—p. 526, Yearly report of Atmospheric electricity, temperature, and humidity for 1867; *Wislizenus.*—p. 532, On the character of the persistent snow accumulations in the Rocky Mountains, and certain features of the Alpine Flora; *Parry.*—p. 557, Analysis of the Rock Salt of Louisiana; *Sander.*—p. 561, Observations of Meteoric Showers; *Hayes.*—p. 562, On the dyas in Nebraska, (translation by N. Holmes); *Marcou.*—p. 565, Remarks on the Loess and Drift of Missouri and Illinois, and upon the Big Mound at St. Louis; *Holmes.*

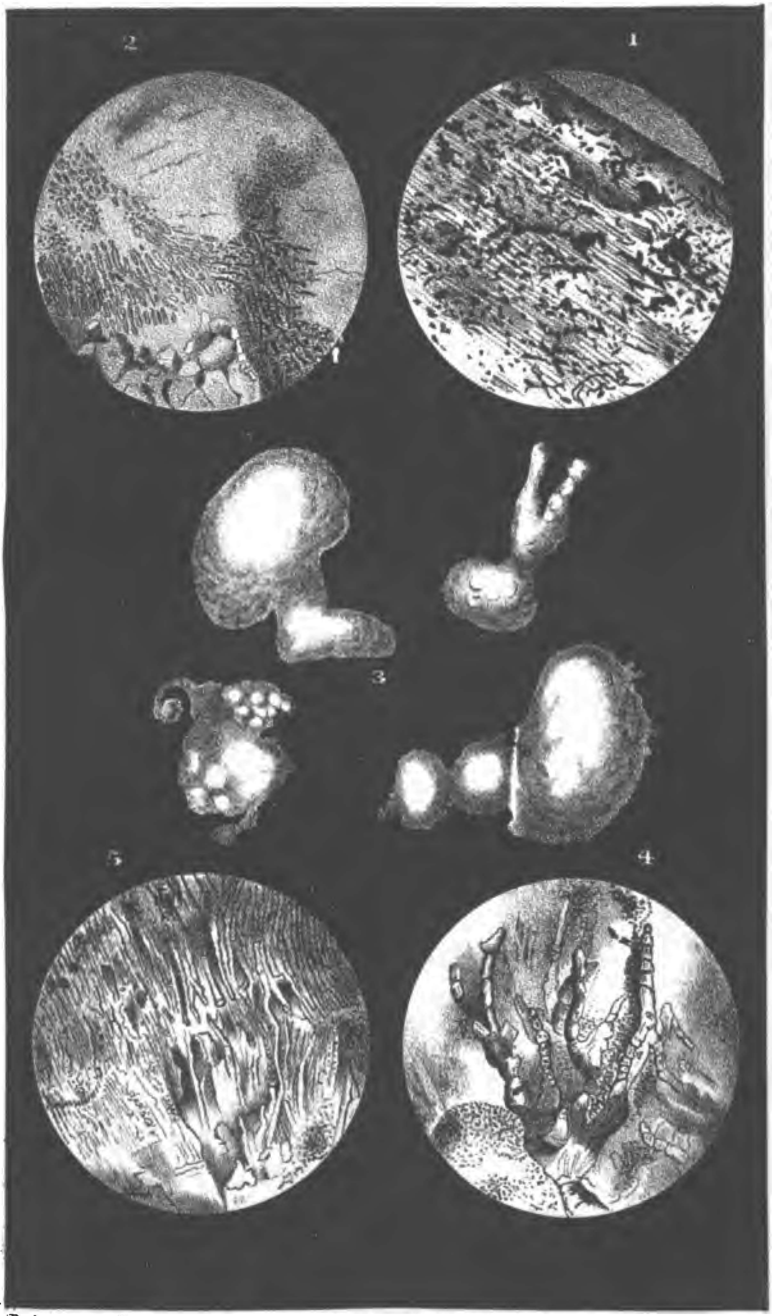
TRANSACTIONS OF THE AMERICAN ENTOMOLOGICAL SOCIETY, Vol. II, No. 1.—Page 1, Catalogue of a collection of Hymenoptera from Mexico; *E. T. Cresson.* —p. 49, New Coleoptera collected between Kansas and Fort Craig, New Mexico, *J. L. Le Conte.*—p, 67, Notes on the N. A. Lepidoptera in the British Museum, and described by Mr. Francis Walker; *A. B. Grote* and *C. T. Robinson.*—p. 89, List of the Ichneumonidæ of North America, with descriptions of new species; *E. T. Cresson.*

PROCEEDINGS ESSEX INST., Vol. v, No. vii.—Page 233, Flora of the Hawaiian Islands; *H. Mann.*—p. 249, Catalogue of the Birds contained in the Museum of the Essex Institute; *Elliot Coues.* Naturalist's Directory, Part II, North America and the West Indes; Pages 57–64; Mollusks concluded, Radiates, Protozoans, Parasites, and additional names in the departments already reviewed.



Roberts & Reinhold Lith Montreal

EOZOON CANADENSE.



Roberts & Reinhold, lith. Montreal.

EOZOON CANADENSE.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. XXVIII.—*On the molecular structure of Uric Acid and its derivatives;* by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.

IN the following memoir I shall endeavor to show that uric acid and its derivatives are members of series the limiting terms of which are on the one side polymeric forms of cyanic acid, and on the other alcohols or the corresponding hydrocarbons. I shall adopt with some slight modifications the notation of Frankland, and shall throughout consider carbon as the principal determinant.

To the molecule of free cyanic acid I give the formula

$$\mathrm{N}\equiv\text{Ꞓ}-\mathrm{Ho}$$

in which all the bonds are saturated. By metastasis* this formula may become in combination

$$\underset{|}{\mathrm{N}}=\underset{|}{\text{Ꞓ}}-\mathrm{Ho}, \quad \text{or} \quad \overset{|}{\underset{|}{\mathrm{N}}}-\overset{|}{\underset{|}{\text{Ꞓ}}}-\mathrm{Ho},$$

and in combination I give the body, $\mathrm{N}\text{Ꞓ}\mathrm{Ho}$, the name of cyanyl. If we replace the hydroxyl by hydrogen we have

$$\mathrm{N}\equiv\text{Ꞓ}-\mathrm{H}$$

which in the isolated state represents a molecule of cyanhydric acid, and which by metastasis gives

$$\underset{|}{\mathrm{N}}=\underset{|}{\text{Ꞓ}}-\mathrm{H}, \quad \text{and} \quad \overset{|}{\underset{|}{\mathrm{N}}}-\overset{|}{\underset{|}{\text{Ꞓ}}}-\mathrm{H}.$$

* I venture to introduce this term to indicate a change or transposition of the units of combining power.

In a state of combination the body, NЄH, may be termed prussyl. With these preliminaries it is easy to see that cyanyl and its derivatives must possess a remarkable aptitude for the formation of polymeric bodies, since the first and last terms of any compound may develop at least two free units of combining power. Thus dicyanic acid when free or saturated may be

$$\begin{array}{ccc} \begin{array}{l} \mathrm{N{=}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{=}Є{-}Ho} \end{array} & \text{or} & \begin{array}{l} \mathrm{N{-}Є{-}Ho} \\ \;\|\quad\;\| \\ \mathrm{N{-}Є{-}Ho,} \end{array} \end{array}$$

but when combined may be

$$\begin{array}{cccc} \begin{array}{l} \mathrm{N{=}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho,} \\ \;|\quad\;| \end{array} & \begin{array}{l} \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho,} \\ \;|\quad\;| \end{array} & \begin{array}{l} \quad\;\;| \\ \mathrm{N{-}Є{-}Ho} \\ \;\|\quad\;| \\ \mathrm{N{-}Є{-}Ho,} \\ \quad\;\;| \end{array} & \begin{array}{l} \;| \\ \mathrm{N{-}Є{-}Ho} \\ \;|\quad\;\| \\ \mathrm{N{-}Є{-}Ho.} \\ \;| \end{array} \end{array}$$

In like manner tricyanic or cyanuric acid has in the free state a molecular structure represented by the formula

$$\begin{array}{l} \mathrm{N{=}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{=}Є{-}Ho.} \end{array}$$

Here again we may have by metastasis either

$$\begin{array}{ccc} \begin{array}{l} \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho} \\ \;|\quad\;| \end{array} & \text{or} & \begin{array}{l} \mathrm{N{=}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{N{-}Є{-}Ho.} \\ \;|\quad\;| \end{array} \end{array}$$

From this it is easily seen that there is no limit, theoretically, to the polymeric forms of cyanyl. Of these cyamelid is doubtless one of the highest as indicated by its remarkable fixedness.

In the acids polymeric with cyanic acid cyanyl may be regarded as the acidifying term. Its quantity therefore determines the basicity of the acid, and its function is the same as that of oxatyl, $\begin{array}{l} \quad\;\;| \\ \mathrm{Θ{-}Є{-}Ho,} \\ \;|\quad\;| \end{array}$ in organic acids not containing nitrogen.

Taking cyanic acid, the lowest term in the series, as the starting point, I propose for urea the formula *

$$\begin{array}{l} \mathrm{N{=}Є{-}Ho} \\ \;|\quad\;| \\ \mathrm{H\;\;N{=}H_2,} \end{array}$$

which affords a better explanation both of the formation of urea from cyanic acid and ammonia, and of its decomposition into cyanic acid, biuret, cyanuric acid, &c., than the commonly received view which makes urea carbamid $N_2\left\{\begin{array}{l}(ЄΘ)\\ H_4.\end{array}\right.$ Upon this view biuret becomes

* Since the above was written I find that this formula has been proposed by Wanklyn and Gamgee. Journal of Chem. Society, vi, p. 31.

$$\begin{array}{ccccc} \mathrm{N} & = & \text{Є} & - & \mathrm{Ho} \\ \mid & & \mid & & \\ \mathrm{N} & - & \text{Є} & - & \mathrm{Ho} \\ \mid & & \mid & & \\ \mathrm{H} & & \mathrm{N} & = & \mathrm{H_2}. \end{array}$$

Its formation from urea and relations to cyanic acid are thus clearly shown. No higher terms in this series are known, but we ought to obtain hereafter triuret, tetruret, &c., corresponding to tricyanic, tetracyanic acids, &c., just as biuret corresponds to dicyanic acid. Thus triuret would be

$$\begin{array}{ccccc} \mathrm{N} & = & \text{Є} & - & \mathrm{Ho} \\ \mid & & \mid & & \\ \mathrm{N} & - & \text{Є} & - & \mathrm{Ho}. \\ \mid & & \mid & & \\ \mathrm{N} & - & \text{Є} & - & \mathrm{Ho} \\ \mid & & \mid & & \\ \mathrm{H} & & \mathrm{N} & = & \mathrm{H_2}, \end{array}$$

and might perhaps be formed by heating together one molecule of dicyanic acid, or two of cyanic acid, and one of urea.

Guanidin and methyluramin appear also to belong to the simple cyanic acid type, their formulas being taken respectively as

$$\underset{\text{Guanidin.}}{\mathrm{NH}=\text{Є}=(\mathrm{NH_2})_2} \qquad \text{and} \qquad \underset{\text{Methyluramin.}}{\mathrm{N}(\text{Є}\mathrm{H_3})=\text{Є}=(\mathrm{NH_2})_2}$$

Dicyanic acid upon my view forms the first or completely nitrogenized term of the following series:

$$\begin{array}{ccccc} \mathrm{N}=\text{Є}-\mathrm{Ho} & \Theta=\text{Є}-\mathrm{Ho} & \Theta=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{H} \\ \mid\quad\mid\qquad & \mid\qquad & \mid\qquad & \mid\qquad & \mid\qquad \\ \mathrm{N}=\text{Є}-\mathrm{Ho} & \Theta=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{H} \\ \text{Dicyanic acid.} & \text{Oxalic acid.} & \text{Glycolic acid.} & \text{Glycol.} & \text{Hydrocarbon.} \end{array}$$

If we replace hydroxyl in dicyanic acid by hydrogen and its derivatives, we have the following possible compounds:

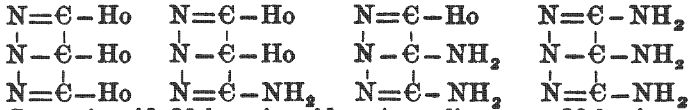
$$\begin{array}{cccc} \mathrm{N}=\text{Є}-\mathrm{Ho} & \mathrm{N}=\text{Є}-\mathrm{H} & \Theta=\text{Є}-\mathrm{Ho} & \Theta=\text{Є}-\mathrm{H} \\ \mid\quad\mid\qquad & \mid\quad\mid\qquad & \mid\qquad & \mid\qquad \\ \mathrm{N}=\text{Є}-\mathrm{H} & \mathrm{N}=\text{Є}-\mathrm{H} & \Theta=\text{Є}-\mathrm{H} & \Theta=\text{Є}-\mathrm{H} \\ \text{Unknown.} & \text{Unknown.} & \text{Glyoxalic acid.} & \text{Glyoxal.} \end{array}$$

To these I may be permitted to add the following which, although, with the exception of glycocoll, not derivatives of uric acid, may be regarded as cognate bodies:

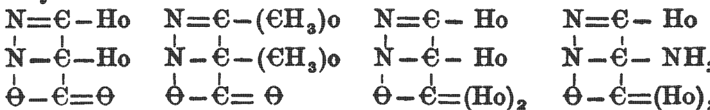
$$\begin{array}{ccc} \mathrm{NH}=\text{Є}-\mathrm{Ho} & \mathrm{N}(\text{Є}\mathrm{H_3})=\text{Є}-\mathrm{Ho} & \mathrm{NH}=\text{Є}-(\text{Є}_7\mathrm{H_5}\Theta) \\ \mid\qquad & \mid\qquad & \mid\qquad \\ \mathrm{H_2}=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{Ho} \\ \text{Glycocoll.} & \text{Sarkosin.} & \text{Hippuric acid.} \end{array}$$

$$\begin{array}{cccc} (\mathrm{NH})-\text{Є}=\mathrm{NH} & (\mathrm{NH})-\text{Є}=\mathrm{NH} & \mathrm{NH}-\text{Є}=\mathrm{NH} & \mathrm{NH}-\text{Є}=\mathrm{NH} \\ \mid\quad\mid\qquad & \mid\quad\mid\qquad & \mid\quad\mid\qquad & \mid\quad\mid\qquad \\ \mathrm{NH}-\text{Є}-\mathrm{Ho} & \mathrm{N}(\text{Є}\mathrm{H_3})-\text{Є}-\mathrm{Ho} & \mathrm{NH}-\text{Є}\diagdown_{\Theta} & \mathrm{N}(\text{Є}\mathrm{H_3})-\text{Є}\diagdown_{\Theta} \\ \mid\qquad & \mid\qquad & \mid\ \diagup\qquad & \mid\ \diagup\qquad \\ \mathrm{H_2}=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є}-\mathrm{Ho} & \mathrm{H_2}=\text{Є} & \mathrm{H_2}=\text{Є} \\ \text{Glycocyamin.} & \text{Kreatin.} & \text{Glycocyamidin.} & \text{Kreatinin.} \end{array}$$

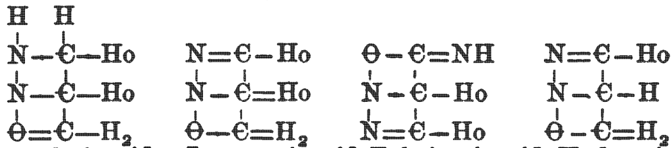
Tricyanic or cyanuric acid forms the first term of the following series:



Cyanuric acid. Melanuric acid. Ammelin. Melamin.

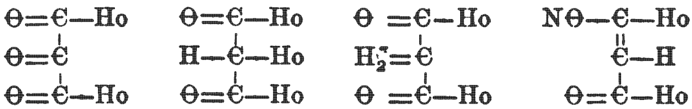


Parabanic acid. Cholestrophane. Oxaluric acid. Oxaluramid.

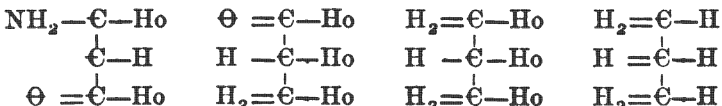


Glycoluric acid. Lantanuric acid. Fulminuric acid. Hydantoin.

Of these bodies melanuric acid, ammelin and melamin were first derived from cyanuric acid by Kekulé.*

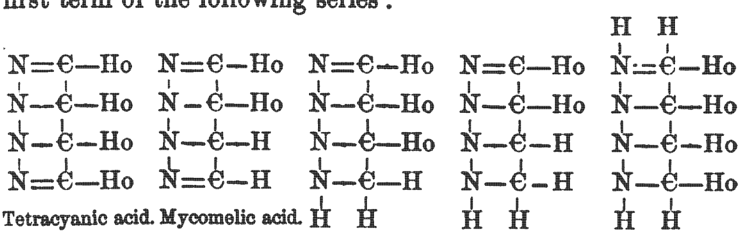


Mesoxalic acid. Tartronic acid. Malonic acid. Nitrosomalonic acid.

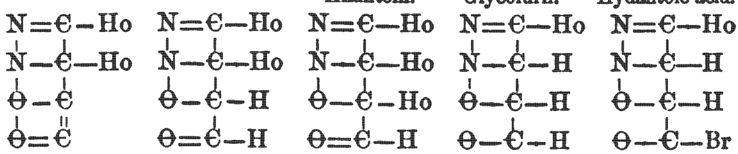


Amidomalonic acid. Glyceric acid. Glycerin. Hydrocarbon.

Tetracyanic acid, $C_4N_4H_4O_4$, has not yet been obtained. I consider its existence as at least probable, and take it as the first term of the following series:

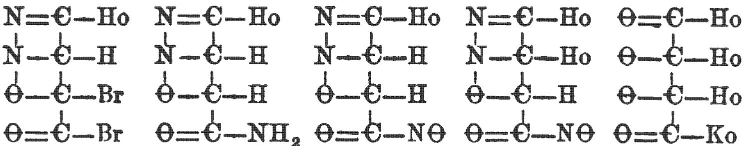


Tetracyanic acid. Mycomelic acid. Allantoin. Glycoluril. Hydantoic acid.

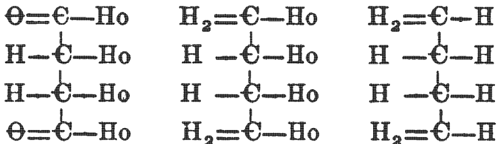


Alloxan. Dialuric acid. Alloxanic acid. Barbituric acid. Bromobarbituric acid.

* Lehrbuch, i, p. 353.

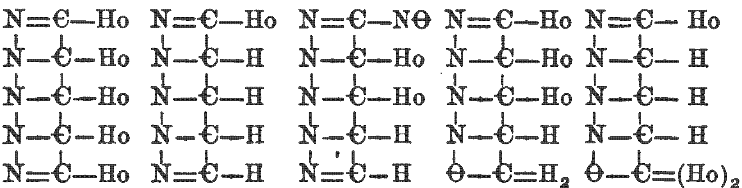


Bibrombarbituric acid. Uramil or Dialuramid. Dialuric acid. Dilituric acid. Dioxalate of potassium.

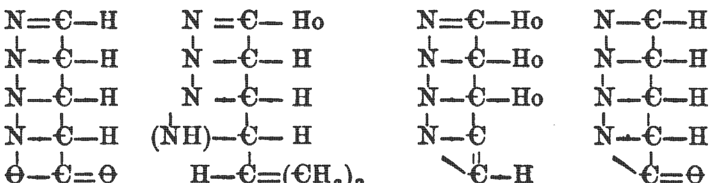


Tartaric acid. Erythrite. Hydrocarbon.

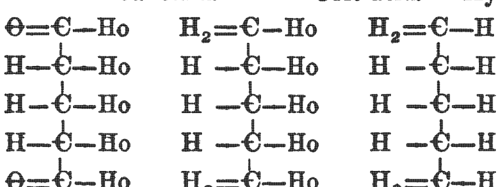
From the hypothetical pentacyanic acid we may derive the following bodies:



Pentacyanic acid. Guanin. Urocyanic acid.* Uroxanic acid. Pseudo-uric acid.



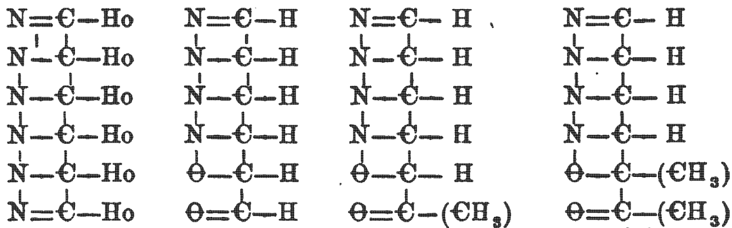
Xanthin. Caffeidin. Uric acid. Hypoxanthin.



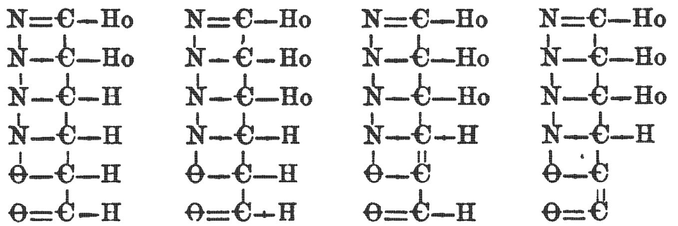
Aposorbic acid. Unknown alcohol. Hydrocarbon.

From hexacyanic acid we may derive the following:

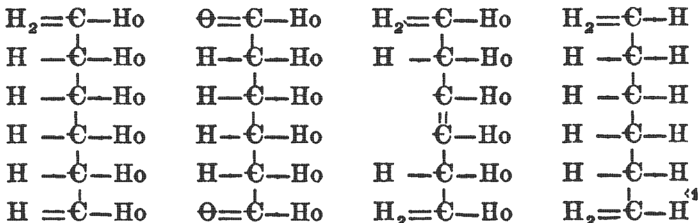
* I have given this name to a new acid obtained by the action of the alkaline nitrites upon uric acid and also, as I believe, upon cyanuric acid.



Hexacyanic acid. Unknown. Theobromin. Theine.

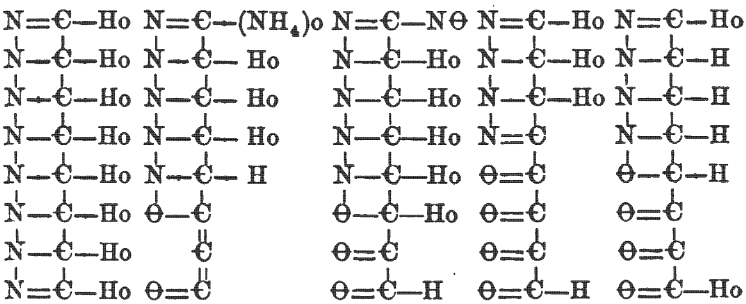


Allituric acid. Dilituric acid. Oxalantin. Leucoturic acid.



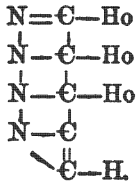
Mannit. Saccharic acid. Glucose. Hydrocarbon.

So far as I am aware no substance derived from uric acid contains 7 atoms of carbon, and it is therefore unnecessary in the present state of our knowledge to assume the existence of heptacyanic acid. The derivatives of octocyanic acid are on the contrary, though not very numerous, of much interest. They are as follows:

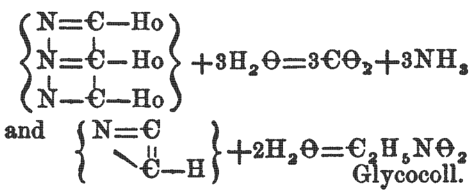


Octocyanic acid. Murexid. Violantin. Alloxantin. Hydurilic acid.

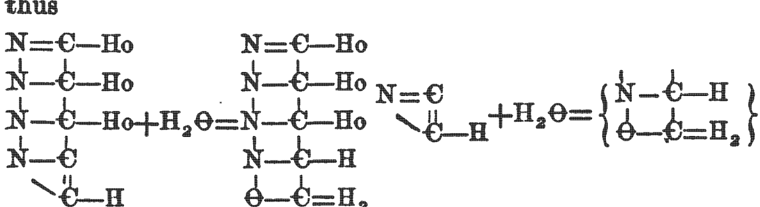
To the theory of the molecular structure of the members of the uric acid series which I have here presented, it may be justly objected that it is to a certain extent arbitrary, since it is easy to see that in many cases the elements admit of a different arrangement. I admit the force of the objection, yet it will be found, I think, upon examination, that there is less room for variation in the arrangements than might at first be supposed. Moreover, I have been guided as far as possible by the known syntheses of some substances and products of decomposition of others. Thus in the case of uric acid, Strecker has recently found that by the action of iodhydric acid it is resolved into one molecule of glycocoll, three of carbonic acid and three of ammonia, so that it may be regarded as derived from cyanuric acid and glycocoll in the same sense in which hippuric acid is derived from glycocoll and benzoic acid. The structural formula which I have given explains the decomposition effected by Strecker very simply, for we have



Now



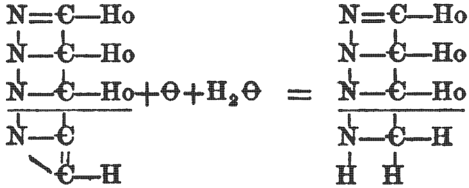
In like manner it is easily shown that uroxanic acid is derived from uric acid by the addition of one atom of water; thus



Uric acid. Uroxanic acid.

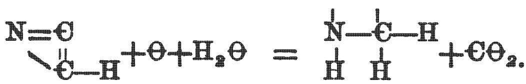
I have here taken uroxanic acid with the formula $\mathrm{Є}_5\mathrm{N}_4\mathrm{H}_6\Theta_4+2\mathrm{H}_2\Theta$ instead of $\mathrm{Є}_5\mathrm{N}_4\mathrm{H}_{10}\Theta_6$ which is usually attributed to it.

The formation of allantoin from uric acid may be explained as follows:

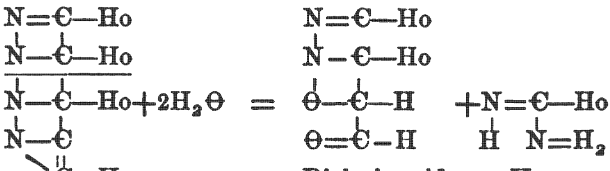


Uric acid. Allantoin.

Here also the action is only exerted upon the glycolic element of the uric acid, since obviously

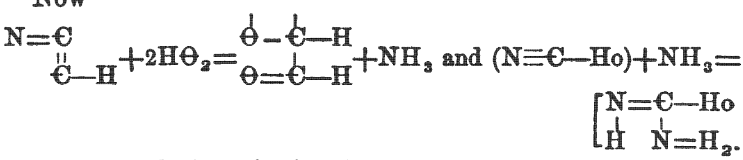


In the formation of dialuric acid from uric acid an atom of cyanyl also enters into the reaction:

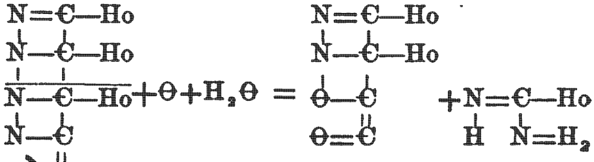


Dialuric acid. Urea.

Now

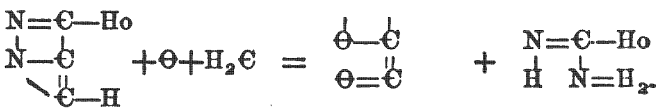


In the oxydation of uric acid to form alloxan an atom of cyanyl is involved; thus

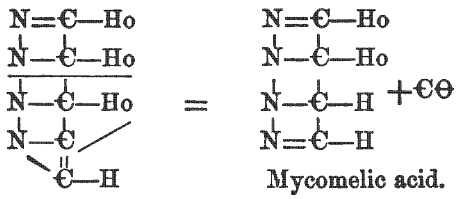


Alloxan. Urea.

Here



In the case of mycomelic acid we have



Mycomelic acid.

To explain all the transformations of the members of the uric acid series would require an inordinate extension of this paper. The reactions indicated by the following equations appear to me to find a ready explanation in the formulas which I have given above, and will serve to facilitate comparison :

$$C_4H_2N_2O_4 + H_2 = C_4N_2H_4O_3$$
Alloxan. Dialuric acid.

$$2C_4H_2N_2O_4 + H_2 = C_8H_4N_4O_7 + H_2O$$
Alloxan. Alloxantin.

$$C_4H_2N_2O_4 + C_4N_2H_4O_4 = C_8H_4N_4O_7 + H_2O$$
Alloxan. Dialuric acid. Alloxantin.

$$C_4H_2N_2O_4 + H_2O = C_4H_4N_2O_5$$
Alloxan. Alloxanic acid.

$$C_4H_2N_2O_4 + O = C_3H_2N_2O_3 + CO_2$$
Alloxan. Parabanic acid.

One transformation deserves especial notice from its bearing upon the theory. By the action of cyanic acid upon dialuramid Baeyer obtained pseudo-uric acid, the equation expressing the reaction being

$$C_4H_5N_3O_3 + CNHO = C_5H_6N_4O_4.$$

Now in this case we have a direct passage from a lower to a higher series by simple addition of one molecule of cyanyl. It seems at least probable that further transformations of a similar kind are possible, and that the discovery of pseudo-uric acid furnishes a clue to a great number of syntheses by one or more additions of cyanyl, prussyl and cyanamid. An attentive examination of the structural formulas which I have given will show that in each series many intermediate terms remain to be discovered.

The views which I have here developed in regard to the molecular structure of the uric acid series appear to me to afford a satisfactory explanation of certain peculiarities which have long attracted the attention of chemists. The first to which I shall refer is the remarkable mobility of the constituents, and the great number of compounds which may be derived from a single primitive by the addition, subtraction or replacement of simple radicals such as hydrogen, hydroxyl, &c., and by the

metastases of the combining bonds. Cases of isomerism are not common among the nitrogenous members of the series, but the theory clearly points to their possible existence and simple explanation. That there is a very close connection between the nitrogenous and non-nitrogenous terms of the several groups can hardly I think be denied, and no theory which has hitherto been proposed offers any explanation of this connection. I am disposed to lay some stress upon this point, believing as I do that it contains the key to the explanation of chemical changes occurring within the animal economy, and gives hints which may hereafter be of service in the rational system of therapeutics to which organic chemistry in connection with physiology will ultimately lead.

The attentive study of the polymeric forms of cyanyl will also I think serve to show that a number of derivatives still remain to be formed from each primitive type, since it is clear that hydrogen and hydroxyl may not only interchange places so as to produce new compounds, but may be replaced by (NH_2) and by other residues. I may here state my conviction that every hydrocarbon is one term of a series which may be considered as derived from another term containing prussyl, cyanyl, or polymeric forms of these bodies, and one problem of organic chemistry, not yet solved or even attempted, is the systematic passage from the completely nitrogenized terms through the intervening stages to the simple hydrocarbons, or from these back to the highest nitrogen derivatives. In the simplest known case this has been done; from prussyl in the form of cyanhydric acid we pass to methylamin, from methylamin to methylic alcohol, and from this to marsh gas. Conversely we pass from marsh gas to cyanhydric acid, but in the cases of the higher terms in the series no such systematic transformation is possible in the present state of our knowledge.

In conclusion I may remark that in place of cyanyl and its multiples cyanogen and its multiples might have been assumed as primitives. I am disposed to think, however, that the method of presenting the subject which I have chosen is upon the whole better and simpler for the purpose which I had in view.

Cambridge, Aug. 15th, 1868.

Art. XXIX.—*The Occultator;* by Thomas Hill, Cambridge, Mass.

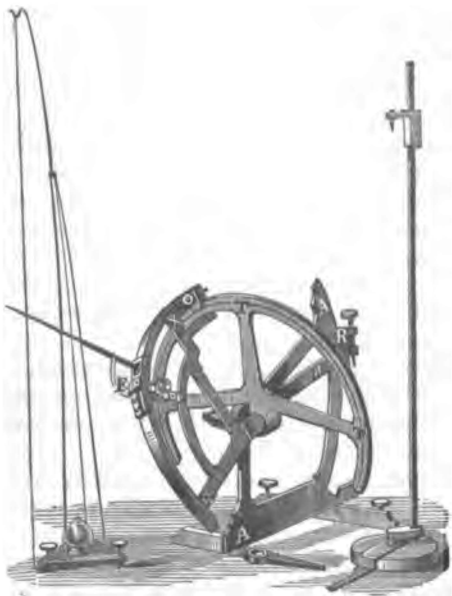
For several years past the appendix to the American Ephemeris and Nautical Almanac has contained a list of occultations, visible in the limits of the United States, west of the Mississippi river, calculated by aid of an instrument invented by me, in the summer of 1842. Having been requested, by one of the editors, to furnish a description of the instrument for the American Journal, I will endeavor to give it in as few words as possible.

From observations on the moon, taken from stations on the surface of the earth, astronomers have, with incredible labor, computed the moon's actual orbit, and predicted, several years in advance, her apparent path in the sky, *as it would be* seen from the center of the earth, were such observations possible; that is, as it would be seen by an observer, flying with great velocity over the earth, with such speed, and in such a path, as to keep the moon always in his zenith.

The problem in calculating an eclipse, or an occultation, is to find from this position of the moon among the stars, as seen by an observer to whom she is in the zenith, where she will appear to an observer in some other position.

The mode in which I solved this problem mechanically will, perhaps, best be understood by following the actual process of my thought in inventing the instrument. A straight wire of infinite length, passing through the centers of the earth and the moon, would mark in the sky, with one end, the moon's real geocentric place, with the other, its opposite pole. A similar wire, passing through the moon's center and the place of the observer, would mark out, with one end her apparent place, with the other its pole. These poles would have the same relative position as the places. Moreover, if the moon be taken as the center of the celestial sphere, these places and poles would keep their right position on a smaller concentric sphere. Let us then take a sphere of about sixty feet radius, or a small portion of its surface, as a chart. Vertically over it, at a distance of sixty feet we place an ideal moon, while upon the chart we will place a model of the earth with a ten-inch radius. The projection of the center on the chart being the moon's real place (or rather its pole), the projection of the observer will be the apparent place (that is, its pole); provided we use the stars instead of the moon's hour angles, &c., which relieves us of the awkward necessity of projecting from the ideal moon and allows us to project vertically.

Of course our model of the earth need be but a skeleton model, and such is "the Occultator." It consists essentially of five parts. I. The moon's meridian, AA, standing vertical upon two feet, and readily arranged by means of a projecting point on one foot and a cross scratch in mica in the other, to stand parallel to the meridians of A.R. in the chart. In the cut the projecting point in the hind foot is out of sight.



II. The equator, TT, which by means of its (axial or polar) arm, RR, and the graduated limb of A, is set to the declination of the moon (in the cut about 12°) so as to keep the ideal moon vertical over the chart).

III. The meridian of the observer, XX, which by the graduated limb of the equator is readily set to the hour angle. In the cut the hour angle is 22^h.

IV. A steel radius sliding in and out of a brass socket, E, which slides on the observer's meridian, XX, to the latitude of the place of observation, in the cut 45°.

V. Some contrivance, two specimens of which are seen by the side of the instrument, for finding the projection of the point of the rod. The one most used is the ruder one at the left, a silk thread stretched on a brass bow which is set on a triangular base, and made normal to the chart by screws in the base. The modes of using the instrument are various.

(*a*) We may draw our chart on a fixed scale of 1 inch $=6'$, and in that case, have fixed meridians drawn, and plot a new path of the moon for each occultation, and take new measures for the semi-diameter, and for the horizontal parallax, represented by the steel radius IV.

(*b*) Or we may fix the length of the steel radius at 10 inches, draw one meridian as an axis, and use Bessel's coördinates to plot the moon's path.

(*c*) But the best practical method yet tried is to plot carefully and permanently the moon's path on the chart with an

hourly motion of 5·6234 inches,—and calculate the proper values for semi-diameter and parallax, and direction of the meridian.

(*d*) Other modes have been tried, but are not worth describing.

It will be observed that the mode *a* is naturally suggested by the train of thought which led to the invention, *b* by the calculation and publication of Bessel's coördinates. The mode *c*, requiring a modification of Bessel's coördinates, was invented by my son, H. B. Hill, who has calculated the occultations for the American Ephemeris. Its advantage is, that it saves the labor of dividing the moon's hourly motion into minutes, which under the other forms is the most tedious part of the process. With the practice which he has had, he is now able to compute by this instrument the times of immersion and emersion, and the angle of emersion for sixteen different places of observation, in less than one hour. The times are supposed to be accurate to the nearest minute, unless the occultation is little more than an appulse.

The instrument was made by J. H. Temple, of Boston, who deserves great credit for his solution of a very difficult mechanical problem,—the adjustment of three motions (declination, hour angle and latitude, I might add a fourth, parallax) on axes passing through a common point, the center of the model earth.

The above description of the process of invention sufficiently demonstrates the principle of the instrument, but it may be demonstrated in quite a different way.

Let P be the elevated pole, z the zenith, S a star, and C the point of upper culmination, let s be a fictitious star, and p a point in the meridian below C, such that $p\,C = P\,z$. Make the angle $C\,p\,s = z\,P\,S$ and $p\,s = p\,C$. The two triangles, $z\,p\,s$, $z\,P\,S$, are evidently symmetrical, so that $s\,z = S\,z$ and the angle $s\,z\,p = z\,S\,P$. That is to say, the altitudes of s and S are equal, and the azimuth of s equals the parallactic angle of S. Hence, if over a horizontal chart a rod, equal in length to the horizontal parallax of S, be elevated to point at s, the projection of the rod will give both the magnitude and the direction of the parallax of S. But it is evident from the construction of the instrument that the steel rod points at s.

Art. XXX.—*On the Amiens Gravel;* by Alfred Tylor, Esq., F.G.S.* Plates III and IV.

I. Introduction.

The exact position, character, and equivalents of the Quaternary deposits of the valley of the Somme have been frequently discussed. On the authority of certain sections and plans of Amiens and Abbeville, the correctness of which will be examined hereafter, theoretical views concerning the relative ages of different parts of the gravel and of different parts of the valley of the Somme have been promulgated by Mr. Prestwich, and repeated by Sir C. Lyell and others.

These geologists have asserted :—

First, that there were two valley-gravels of distinct age at Amiens and Abbeville, one named by them the upper and the other the lower valley-gravel;

Secondly, that the upper gravel was the older of the two; and

Thirdly, that the valley of the Somme was excavated to a depth of 40 or 50 feet since the deposition of the upper valley-gravel, and previously to the deposition of the lower valley-gravel;

Fourthly, that both gravels were fossiliferous, and contained the remains of man, or rather human implements, and bones of extinct mammalia, the lower gravels having the greater number of species of mollusca, the higher gravels containing the greater number of flint implements;

Fifthly, that the height of 70 feet, at which fossiliferous gravels now stand above the level of the Somme, is much beyond the limit of floods, and therefore, that these gravels could only have been deposited at St.-Acheul before the river-channel was cut down to its present level.

The general effect of these assertions was to refer the remains of man found in St.-Acheul back to an indefinite date, separated from the Historical period by an interval during which valleys were excavated or deepened 40 or 50 feet.

In a paper read before this Society in April, 1866,† I suggested that there was evidence of very little weathering or atmospheric action since the date of the drift containing human remains, and that the age of these deposits was close to the Historical period,—also that the upper and lower valley-gravels in the Somme were continuous and of one period.

* From the Quarterly Journal of the Geological Society for May, 1867.

† Quart. Journ. Geol. Soc., vol. xxii, p. 463.

I afterward read, in June, 1866, a statement of what I believed to be the correct interpretation of the Amiens and Abbeville sections, reasserting the continuity of the gravel-deposits on gradual slopes from the higher to the lower levels in the valley, except in rare cases or isolated spots, where the continuity was interrupted or prevented by some upstanding piece of the original rock out of which the valley had been cut, in which case the gravel wraps round the base of the upstanding knoll of chalk. I quoted the section at Montiers as one that shows a direct sequence of gravel from above the railway to the Somme, notwithstanding the version of that locality published by Mr. Prestwich, in which chalk is represented in a position where I could only find gravel.

At the same time attention was drawn by me to the probability of the brick-earth terrace sloping down to the Lea Marshes at Clapton being of the same age as the similarly formed Loess terrace sloping down to the Somme at Amiens. I also asserted that there was good evidence in the direction and gradient of the terrace, in the configuration of the gravel and brick-earth and of the London-clay surface at Clapton, of the water having occupied the whole valley of the Lea at the time of the formation of this Clapton terrace, and also of the water of the river Lea or other rivers having reached very much higher levels in the vicinity at that period, while the Stoke-Newington and Highbury gravels and brick-earth were being deposited.

I still hold these opinions, and am prepared to demonstrate their truth; and I ask the attention of the Society to a restatement of the exact geological facts to be seen in the Somme valley, and to evidence quite independent of that which has been previously submitted to the Society, although to a certain extent going over the same ground.

The conclusions that I arrive at are extremely dissimilar to those of Mr. Prestwich and Sir C. Lyell, and are as follows :—

First, that the surface of the chalk in the valley of the Somme had assumed its present form prior to the deposition of any of the gravel or loess now to be seen there, and in this respect corresponded with all other valleys in which Quaternary deposits of this character are met with.

Second, that the whole of the Amiens-valley gravel is of one formation and of similar mineral character, and contains nearly similar organic contents, the La Neuville, Montiers, and St.-Acheul gravels being of the same age, and capped with a covering of loess also of one age and mineral character, the whole deposit being of a date not much antecedent to the Historical period.

Third, that the gravel in the valley of the Somme at Amiens is partly derived from *débris* brought down by the River Somme and by the two rivers, the Celle and the Arve, and partly consists of material from the adjoining higher grounds, washed in by land-floods,—the immense quantity of chalk present in the gravel having been derived from the latter source. It is where the surface of the chalk is concave that the gravel is thickest.

Fourth, that the Quaternary gravels of the Somme are not separated into two divisions by an escarpment of chalk parallel to the river as has been stated. They would have formed an exception to other river-gravels if this had been the case. The St.-Acheul gravels thin out gradually as they slope from the high land down to the Somme, and they pass away into the Loess formation,—and so also at Montiers.

The Loess deposit, on the contrary, forms a distinct escarpment for many miles along the Somme; and this, I believe, is the bank of the ancient river whose floods produced the St.-Acheul and Montiers gravels.

Fifth, that the existence of river-floods, extending to a height of at least eighty feet above the present level of the Somme, is perfectly proved by the gradual slope and continuity of the gravels deposited by those floods upon the sloping sides of the valley toward the Somme, and also by the Loess or warp, of similar mineral composition and color, extending continuously over the whole series of gravels, and finishing with a well-defined bank near the present stream.

Beds of gravel, brick-earth, and loess, having an even sloping surface from the escarpment of the sides of the valley down to the terrace near the river-bank, are often to be observed near other rivers whose channels bear the same proportion to their valleys that the Somme river bears to its valley, and where gravel- and loess-deposits reach to a height of 100 feet above the present river-levels.

Sixth, that many of the Quaternary deposits in all countries, clearly posterior to the formation of the valleys in which they lie, are of such great dimensions and elevation that they must have been formed under physical conditions very different from our own. They indicate a Pluvial period, just as clearly as the northern drift indicates a Glacial period. This Pluvial period must have immediately preceded the true Historical period.

Since June 1866, I have visited Amiens several times, and compared the gravels as accurately as I could, both as to situation and character, with those of other rivers, of which I have had surveys made; and I hoped to have brought the whole subject of valley-gravels before the Society in the early part of

1868. The announcement of the intended absence from England of Mr. Prestwich, has induced me to describe the Amiens gravels first, in order to have the advantage of his presence at the meeting, reserving my account of other river-gravels and general comparison to a future paper.

The plan and sections of the Quaternary deposits at Amiens illustrating this paper will, I hope, make the actual geographical and geological positions of the Amiens beds quite clear.

It should be mentioned that, in August, 1866, after examining the levels myself, I called at the railway office at Amiens to obtain the precise height of the different points on the railway above the sea, and to get the name of a competent surveyor, and was referred to the chief engineer, M. Guillom. That gentleman only saw me for a few minutes, but kindly promised that if all the points on which I required information were laid down on a plan, he would have the sections carefully taken for me in a few weeks. This plan was accordingly sent to him by me a few days afterward, with the lines marked on which I wished the levels.

The work was more tedious than was expected by M. Guillom, and he did not send me the measured sections until May, 1867. The levels of these sections have been taken with the greatest care, and, I believe, are as precise as any that have been taken for geological purposes; and I am therefore indebted to Mr. Guillom for the means of drawing an exact picture of the surface of the chalk prior, as I believe, to the deposition of any of the valley-gravel or loess at Amiens. The value of this communication therefore much depends on Mr. Guillom's survey.

The heights on the maps and sections are in English feet, the datum line being mean tide at Havre. The scale of the plan (Plate III), is 3⅛ inches to a mile. The vertical scale in Plate IV, is ½ inch to 55 feet, and is three times the horizontal scale. The dotted lines on the plan show the position of five sections, I K, R S, L M, N O P, N Q, nearly at right angles to the river, some of them extending to a height of 200 feet above the sea. The line A B is along the Imperial Road. See Plates III and IV.

II. Description of the Longitudinal Section.

There is also a longitudinal section, divided into three parts on account of the public buildings at St.-Acheul preventing continuous levels being taken. The divisions are C D, E F, and G H; but it will be treated sometimes in this paper as one section, C H. See Plate IV, fig 3, 4, and 5.

It passes through the celebrated pits of St. Acheul, and is bounded by the river Arve, a tributary of the Somme, at its eastern point, C, and by the escarpment of chalk in the Rue de Cagny (700 yards west of the railway-station, Amiens) at its western extremity, point H.

C H is near the Imperial Road, and is parallel to that road, to the railway, and to the river Somme.

Section G H.—The length of G H is 1400 feet. See Plate IV, fig. 5. The highest point is 157 feet above the sea, 79 feet above the river Arve, 3 feet over the highest part of the Imperial road, 61 feet above the rails, and 84 feet above the river Somme at Neuville. The gradient, commencing at point H, 129 feet above the sea, Rue dė Cagny, rises to the east 1 in 30, then 1 in 33, 1 in 35, 1 in 61, and 1 in 100, reaching the well-known section of St.-Acheul Pit, with Roman graves, fossiliferous sand, and wavy marls, at a height of 152½ feet above the sea. A portion of this is shown in Plate IV, fig. 12.

The loess in this section is four feet at the highest, and most easterly point, G, gradually thinning to the west, and ceasing when it reaches H.

The gravel is sixteen feet thick at its most easterly point, G, thinning out as it passes to the west a little before the loess disappears.

The surface of the chalk is 133 feet above the sea at G, and 128 feet at H. The surface falls 1 in 280 to the west.

Section E F.—The surface gradient commences at F, at a height of 156 feet above the sea, and it passes the Cemetery road on the level, and rises at a gradient to the east of only 1 in 700, then falls to the east at 1 in 165 and 1 in 701, reaching the point E at a height of 154 feet above the sea (Plate IV, fig. 4).

The loess is 4 feet at F, thickening to 5 feet at the summit-level of the whole section C H, and then thinning out to 4 feet at E. The regularity of the loess is a very important fact.

The gravel is 17 feet thick at F, and 15 feet at E. The surface of the chalk is 133 feet above the sea at both E and F, showing a perfectly horizontal line, while there is only a variation in the level of the surface-loess of 3 feet in this section, which is 1586 feet long.

Section C D (Plate IV, fig 3).—Section C D commences at D with an elevation of 153¼ feet above the sea; and the gradient falls east at 1 in 157, then rises to the east 1 in 80, then falls to the east 1 in 40 and 1 in 300. Here the tramway (Plate IV, fig. 1) crosses the Imperial Road, and some very extensive gravel pits are now being worked for ballast.

The gradient continues falling east 1 in 88, 1 in 180, 1 in 160, 1 in 41, 1 in 33, and rising 1 in 200 to the east, where it reaches the escarpment at a height of 142¼ feet above the sea. The loess is here 5 feet thick, and the gravel 2 feet, according to Mr. Guillom's survey; but I found only two or three feet where I observed it. The loess is 5 feet thick near the tramway, and 4 feet at the point D. The gravel is 13 feet thick at D, and 10 feet thick at the tramway, thinning out as it approaches the escarpment on the east, as it did on the west. The surface of chalk is horizontal throughout this section also up to the escarpment.

At the escarpment, the chalk falls to the east 52½ feet in a distance of 106, or at an angle of 45° and gradient of 1 in 2 nearly. The line of slope of this escarpment is remarkably straight in many places, and quite free from gravel or loess. Then there follows a flat terrace of loess, 60 feet wide, then a slope toward the river, of 1 in 30, and then 1 in 4, until we reach the marsh at a height of 76⅓ feet above the sea.

III. Descriptions of the Transverse Sections.

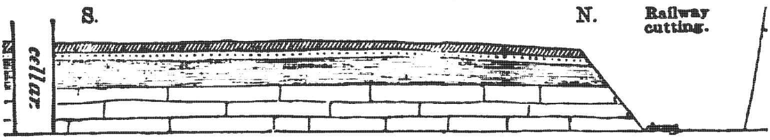
Section I K (Plate IV, fig. 6).—This section commences at the Rue de Cagny, point I, at a height of 200 feet above the sea, and falls to the river and the north at a gradient of 1 in 32, 1 in 28, 1 in 22, 1 in 18, 1 in 54. It then rises to the north at 1 in 162, and crosses the tramway ballast-pit at a level of 153½ feet above the sea, and the Imperial Road at a height of 153 feet above the sea; it then rises to the north at a gradient of 1 in 20, reaching 156 feet above the sea, then falls toward the river at 1 in 42, 1 in 100, rises 1 in 87, falls 1 in 67, 1 in 65, 1 in 50, until in reaches the railway-cutting, at a height of 138 feet above the sea. The cutting happens to be in the escarpment of the ancient chalk valley, in which the valley-gravel has been deposited; and the surface of the gravel follows the contour of the ground, and falls at a gradient of 1 in 8, and then 1 in 7, declining 47 feet in a distance of 360 feet. The surface then falls more gently to the river at 1 in 36, 1 in 34, until it reaches the Somme.

At the point I in the Rue de Cagny the loess is 3 feet thick, and near the Imperial Road it is 8 feet thick; at one point it gradually thins out toward the river and railway, and at the railway-cutting the loess is only 2 feet thick. I do not know the thickness on the north side of the railway; but as the gravel thins out rapidly, the loess is no doubt from 10 to 12 feet thick in some points. The gravel at the point I is 5 feet thick; it increases to 10 feet thick as it approaches the Imperial Road, and after passing that at a height of 148 feet above

the sea, it gradually thins away until it is only 3 feet thick at the south side of the railway-cutting, and soon merges into the loess on the steep incline on the north side of the railway.

The surface of the chalk at the Rue de Cagny near the point I is 195 feet above the sea; it falls to 136 feet above the sea where it passes under the Imperial Road, and then becomes nearly horizontal, only falling 3 feet until it reaches the railway-cutting.

Fig. 1.—*Section at La Neuville, showing the Loess resting immediately on the Chalk.*



The slope then becomes rapid again, and it probably falls at a gradient of 1 in 4 for some distance, and then becomes horizontal again at the river Somme.

The loess is clearly seen in the railway-cutting and at one cellar (Plate III, C)* in Neuville (fig. 1), resting on chalk without any intermediate gravel, C on the plan; but I have left the junction between the loess and gravel undefined in the lower part of the section I K, as I could not put the junction in accurately.

If a straight line be drawn from point I on the Rue de Cagny to K on the Somme, it will pass 32 feet below the top of the railway-cutting along the line I K, and it will pass over the chalk at the Imperial Road at a height of 17 feet, showing that the surface of the chalk between those two points is concave.

Section L M.—(Plate IV, fig. 7.)—This section commences at the point beyond the Rue de Cagny, at a height of 187 feet above the sea; the gradient dips northward toward the river, and falls 1 in 37, until it passes the Rue de Cagny at a height of 160 feet above the sea. It passes through a part of the great St.-Acheul pit, with a gradient of 1 in 15, 1 in 40, 1 in 70, 1 in 130. Here it crosses the Imperial Road and falls to the north with a gradient of 1 in 600, 1 in 300, 1 in 40, 1 in 688, 1 in 43, to the La Neuville Railway ballast-pit close to the railway workshops, where it reaches a height of 132 feet above the sea. The ground is quarried out; but the surface apparently dipped north at a gradient of 1 in 12, then rose 1 in 33, then fell 1 in 7, horizontally crossing the railway-cutting at a height of 107 feet, 11½ feet above the rails.

* The letter C in La Neuville must be distinguished from the letter **C** in Longueau on the same Plate III.

The ground then falls north, at a gradient of 1 in 21, forming the top of the loess terrace, at 95 feet above the sea. The escarpment of the loess terrace is here at a very steep angle, falling 12 feet in a horizontal distance of 14 feet, then at a gradient of 1 in 71 to the first brook, then level to the Somme.

The loess is 4 feet thick at the Rue de Cagny, in the section L M and the same thickness at the Imperial Road. At the escarpment of the chalk it is only 2 feet thick, and seems to appear again at the terrace in considerable thickness, at a height of 95 feet above the sea.

The gravel is 13 feet thick at the Rue de Cagny, nearly 20 feet thick in the St.-Acheul pit, and runs out to 6 feet at the Railway-works ballast- and chalk-pit at the escarpment. I have no section of gravel further north than this pit.

If a straight line be drawn from the Rue de Cagny to the Somme, along the line L M, it passes the Imperial Road on the level, and is 15 feet below the surface at the Railway-works ballast-pit; so the surface is convex at that point.

The convexity of the chalk on the same line is 14 feet at the ballast pit.

Section N O P.—Plate IV, fig. 8.)—This section commences at the Ferme de Grâce, point N, at a height of 201 feet above the sea, and goes along the road to Montiers by the line N O P as far as O. The first gradient is 1 in 33, north; 1 in 90, 1 in 100, 1 in 105, 1 in 110, 1 in 110, 1 in 110, 1 in 57, 1 in 60, 1 in 70, 1 in 60. Here it crosses the junction of two roads at a height of 155 feet above the sea. Then follow on north 1 in 60, 1 in 27, 1 in 40, 1 in 60, to the point O, at a height of 120 feet above the sea; then 1 in 30 and 1 in 75 to the railway, 1 in 33 to the Imperial Road, then 1 in 56, 1 in 50, 1 in 231, and it crosses the top of the escarpment of loess at a height of 81 feet above the sea. Then the face of escarpment falls 16½ feet in 18 feet, then is horizontal to the river.

If a line be drawn from the Point N to the river Somme along the line N O P, it will pass under the junction of two roads at a height of 142 feet above the sea, or 15 feet under the roads. It will pass 10 feet above the rails, and 2 feet above the Imperial Road; so that the extreme convexity of the surface at any point of the line of 7458 feet is only 15 feet. This is important, as the section has been represented as enormously convex by previous writers.

The surface of chalk at the junction of the two roads is 142 feet above the sea, and is therefore only 6 feet above a straight line drawn through N O P. The surface of the chalk at the railway is 23 feet below a straight line drawn from the Ferme de Grâce 201 feet above the sea to the Somme (at a height of 61

feet above the sea); a straight line from the Ferme de Grâce, 201 feet above the sea to the Somme, 61 feet above the sea, passes over the railway 8 feet above the rails, and 23 feet above the surface of the chalk at that point, so that the surface of the chalk is concave to the extent of 23 feet. At the Imperial road the surface of the chalk is concave to the extent of 22 feet, although on the upper part it is convex to the extent of 15 feet.

IV. Characters of the Chalk, Gravel, and Loess.

I will not trouble the Society with the details of Section N Q (Plate IV, fig. 8), but will now proceed to describe the characters of the chalk, the gravel, and the loess, as I have observed them near Amiens.

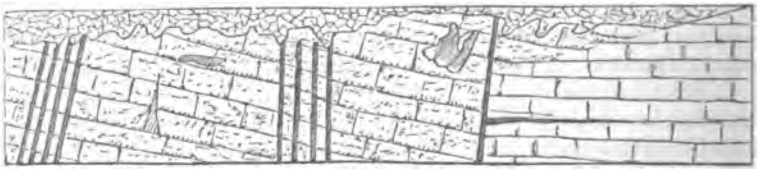
1. *The Chalk.*—The condition of the chalk itself near Amiens is remarkable in some places.

In a railway section near Pont de Metz, about three miles from Amiens and Montiers, the chalk surface slopes northward at an angle of 20°, and is overlain by 20 feet of drift sands dipping 10° N. where they touch the chalk, but filling up the concavities of the chalk, and having their upper surface sloping northward at an angle of 3°.

At Pont de Metz the chalk is covered with a drift chalk-marl, and with beds of chalk rubble and chalk pellets, with very little mixture of sand or clay, 15 to 20 feet thick.

Near Guigencourt, a quarry in the chalk on the plateau, about four miles south of Montiers, the chalk is very much split up by joints lying at an angle of eighty degrees, or very nearly vertical, and also nearly at right angles to the planes of bedding of the chalk. (Fig. 2.)

Fig. 2.—*Section exposed in a Chalk-quarry near Guigencourt.*



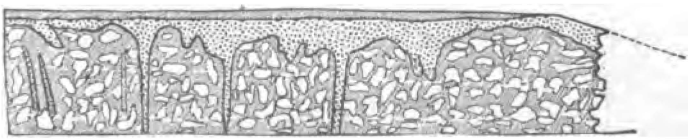
These joints are now in many cases fissures two or three inches wide, and extending to a considerable depth; but they are filled up with a fine brown loess, which seems as if injected into them; for I observed in one or two cases that a vein of two or three inches thick had entered a horizontal joint, and passed along that in a horizontal direction, thining out to only half an inch. I give a sketch of this chalk-quarry. This sys-

tem of joints would very much facilitate the formation, or rather the separation, of the chalk into rectangular and imperfect spheroids, such as are seen in the quarries behind St.-Acheul and Longueau, where some decomposing agency has acted upon the chalk itself with considerable effect.

In the drawing (fig. 3) made of the condition of the surface

Fig. 3.—*Section along the St. Acheul and Longueau Road.*

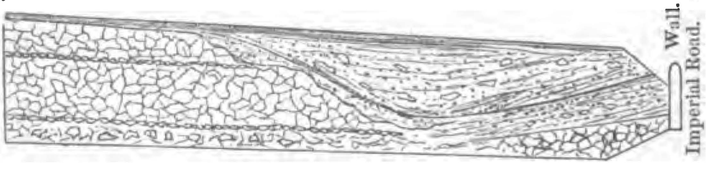
W. E.



of the chalk (and to a depth of 20 feet) along the Saint-Acheul and Longueau road, running east and west, I found the sand-pipes in the chalk very close together, and filled with brown loess and gravel. There are some large pipes in the eastern escarpment in M. Dailli's garden, close to the road; but they decrease toward Cagny on the escarpment of chalk trending southward there exposed, and also in the escarpment of chalk trending northward. (See fig. 4.) I did not observe any de-

Fig. 4.—*Section in M. Dailli's Garden showing decomposed Chalk.*

S. N.



composed chalk in the railway-cutting or quarry between Longueau and La Neuville, nor at the ballast-pit in the chalk near La Neuville at the railway workshops, Amiens. The surface of the chalk, however, is irregular, and covered with gravel, but without deep pipes.

The drawings of the chalk-quarry, fig. 2, and in M. Dailli's garden, north of C, fig. 4, will explain the remarkable character of the decomposition that has affected the chalk. Not only has chalk been removed by the chemical action which bores pipes in it, but the loess appears to have followed closely, penetrating through the mass for many feet, occupying the vacant space made by the destruction of the calcareous matter in many places, or uniting with it, and making a kind of Combe Rock.

The harder pieces of chalk are left, often in a boulder-like form (as drawn), with slightly rounded or abraded corners, the chalk between large pieces then being loose and friable, and

marly in color, often mixed with loess, and with ferruginous stains. When the chalk is quarried, the large masses fall down like boulders, and are used for purposes of masonry, untouched by the quarryman. The hard pieces of chalk project beyond the soft matrix in which they are inclosed, like the flints upon the Brighton cliff, making a serrated face. The largest piece that has fallen out is only about three feet long, according to M. Dailli, who has quarried thousands of tons of chalk without meeting with a larger mass. There is a pipe, ten feet in diameter, in M. Dailli's garden, and the depression in the chalk at the north-east corner has a pipe-like form.

The lines of large flints which traverse the whole of the escarpment horizontally are perfectly *in situ*.

That this decomposition of the chalk *in situ* has some relation to the physical circumstances following the deposition of gravel at St.-Acheul is, I think, probable, as some part of the drainage from above St.-Acheul would pass through the escarpment in question in order to get to the marshes, and the action which has caused the removal of the chalk must have acted with great intensity on the high land adjoining, so that the current was from above downward. About one-eighth of the St.-Acheul gravel consists of chalk in the form of large pieces averaging 4 inches diameter, of chalk pellets from $\frac{1}{8}$ to $1\frac{1}{2}$ in diameter, and of chalk finely divided and mixed with clay.

Where we can see the chalk near C, it is so perforated by pipes and separated into small pieces that it seems prepared for a rapid denudation if attacked by water with any vigor; and if this was the condition of the chalk also at higher levels near St.-Acheul and Montiers, we can account for the large quantity of chalk contained in the Amiens gravels.

The fall of the Somme from Longueau to Montiers is fifteen feet, the river flowing from southeast to northwest nearly, at a gradient of only 1 in 1520. The rails are 96 feet at La Neuville, and 99 feet at Montiers, above the sea-level.

By referring to the sections, C D, E F, G H, which are parallel with the river Somme and the Imperial road, it will be seen that on a line from east to west, 1644 yards long, from the eastern escarpment of the chalk east of St.-Acheul to the western escarpment of the chalk near the northern termination of the Rue de Cagny, the surface of the chalk is extremely regular and horizontal.

The highest point of the chalk on the line C H is only three feet higher than the lowest point on that line.

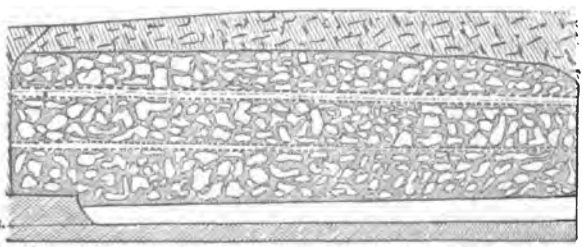
There is a steep escarpment, 50 feet high, at Longueau, of bare chalk facing the east, and an escarpment 30 feet high, near

the Rue de Cagny, of bare chalk facing the west. The outcrop of chalk is marked on the plan, in order to be well seen.

The contrary is the case with the sections from south to north. The escarpment of the chalk facing the river Somme

Fig. 5.—*Section of decomposed Chalk exposed in a quarry in the escarpment near* **C**, *with bank of Loess at the base.*

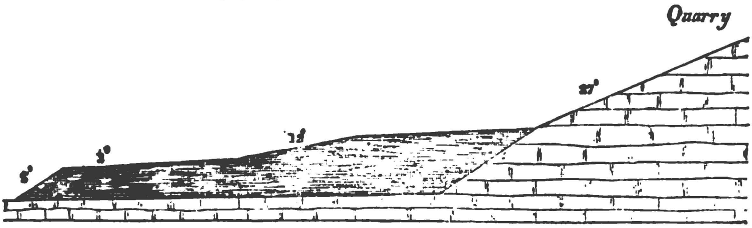
N. S.



Loess. Talus.

Fig. 6.—*Section showing the escarpment of Loess between the Quarry* (fig. 5) *and the Imperial Road.*

E. W.



is not so steep and is therefore nowhere bare, being covered with a Quaternary deposit.

The slope of the chalk from south to north is considerable when compared with the almost perfect horizontality of the chalk in an east and west direction.

Thus we have a slope or gradient of 1 in 33, or of $2\frac{3}{4}°$, beginning at the point L on the line L M, 175 feet above the sea, to M on the Somme, at a height of 76 feet above the sea. The distance is 3342 feet between L and M. These escarpments are evidently the sides of lateral valleys, and are not due to the action of the river Somme, but to that of smaller lateral and more rapid streams running into the Somme. The river Arve still approaches closely to the eastern escarpment of St.-Acheul. The western escarpment of St.-Acheul is the side of a valley now dry, but which evidently contained a rapidly flowing stream when the western escarpment was formed.

The gradient of the river Arve is much steeper than that of the Somme ; but the valley at the west of St.-Acheul formerly contained a stream which must have fallen with great velocity,

as the slope af the bottom is 1 in 40, nearly equal to the slope of the chalk itself at St.-Acheul, which is 1 in 33. This river is now dry.

I ask particular attention to the position and level of this dry valley, which is similar in character to those occurring on all chalk-downs and plateaus.

The chalk surface at St.-Acheul is also hollowed out into a valley situated north of the Imperial Road, widening out as it approaches the Somme, after the ordinary manner of valleys.

By the sections through the St.-Acheul pits, we know this valley did not extend south of the Imperial Road; but the eastern boundary of this small valley, only 400 or 500 yards long, is well seen at the La Neuville Eastern Bridge, where the chalk is well exposed in the railway cutting, at a height of 20 feet above the rails, and slopes westward, passing under the rails near the point C in the map, between the lines of section I K and L M.

The surface of chalk is shown in a very clear section on the railway here, covered with twenty feet of loess (fig. 11). The chalk is nowhere naturally exposed. The force of water from St.-Acheul originally hollowed out this small valley in the chalk, which has been partly filled up with gravel and loess; and the surface-drainage of St.-Acheul flows to the river Somme down this valley, over a bed of gravel and loess of some thickness.

There is a very small lateral valley in the chalk, running from St.-Acheul into the now dry valley at the western escarpment, also covered with loess and gravel. The slope of the side of this valley is as much as 6°.

Crossing over from the east of Amiens to the west, we come to the section N O P, which gives us a correct view of the surface of the chalk at Montiers, where fossiliferous gravels were discovered by Mr. Prestwich. (Plate IV, fig. 7.)

The gradients have been already described. Between N and O the surface of the chalk is slightly convex; but between O, a point 120 feet above the Somme, and the Somme itself, the surface of the chalk is concave.

In an elevation of 60 feet, between O and P, the concavity of the chalk is as much as 20 feet, or one-third of the total height. It is in this basin of the chalk that the great gravel-beds of Montiers may be seen, in which 30 feet of gravel and loess is well exposed, south of and close to the Imperial Road.

The fossiliferous gravel extends above the railway; and Mr. Prestwich found shells in a pit which appears to be about 50 feet above the river at Montiers.

The chalk is nearly horizontal beneath the rails for a distance

of 1077 yards between the line of the section N O P, and N Q; at least it is 15 feet below the rails on the line N O P, and 9 feet below the rails at N Q (fig. 12, p. 123). As at St. Acheul, the slope of the surface follows the chalk to some extent, and falls toward the river. The average gradient is $2\frac{1}{4}°$, or 1 in 43, along the line N Q, against a gradient of $2\frac{3}{4}°$, or 1 in 33, at St.-Acheul, along the line L M; but the chalk is convex on the average at L M, and concave on the average at N Q. This, however, requires more explanation.

The surface of the chalk between the line of 200 feet and of 120 feet above the sea is convex on the line N O P, at the maximum to the extent of 14 feet out of 80 feet perpendicular height; and we see very little gravel reposing on the convex surface. On the contrary, in L M, between the 200-feet and 135-feet levels, the maximum concavity is 15 feet; and the great mass of the St.-Acheul gravel is deposited in this hollow.

But when we examine the surface of the chalk between O and P, between the 120-feet and 60-feet levels, we find the chalk surface is concave to the extent of a maximum of 29 feet, out of a total of 60 feet; and, singularly enough, this 29 feet is almost exactly the maximum thickness of gravel and loess in the great pit at Montiers, where a section several hundred yards long is exposed.

On the contrary, between the 130-feet and 76-feet levels on the lines L M and I K, where the surface of the chalk is convex, there is no gravel of any importance.

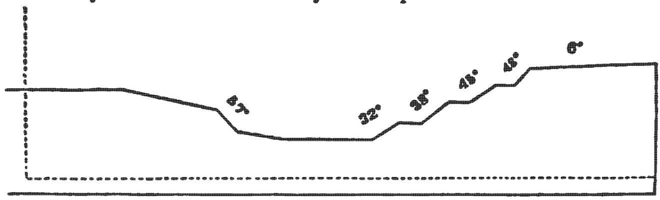
In the section (Plate IV, fig. 1), between L M and I K the chalk is nearly a straight line, falling $2\frac{1}{4}°$ between the 130-feet and 90-feet levels; we have 9 feet of gravel and loess exposed in this favorable position for its accumulation.

When we see the gradual slope of the surface from the point O to the river Somme at P, we are indeed surprised to find the sudden change in gradients in passing southward from O to Renancourt, across the now dry valley leading from Ferrières, (passing by Saveuse) to Amiens, a distance of four or five miles. These escarpments commence near Ferrières, and increase as the bottom of the valley falls in a northeasterly direction toward the river Cette. I measured a section near the Ferme de Grâce, where the side of the dry valley slopes at an angle of 20° to the bottom.

These escarpments are better shown in a section taken from the point O, toward Renancourt, and giving gradients from 30° to 50°, representing flood-lines of former periods, but so sharp in definition that they look like the work of the last winter. (Fig. 7.)

Fig. 7.—*Section across the Saveuse valley.*

380 yards broad. 28 yards deep.



It is obvious that any theory of excavation of the Somme valley, at Amiens, must take into account the condition of the dry Saveuse valley, which is only a type of hundreds of other dry valleys, which formerly were filled with water falling into the Somme and swelling it into a river capable of overflowing St.-Acheul.

In the same way no argument can be satisfactorily applied to the formation of the Somme valley, without considering the relation of the chalk-valleys generally, which resemble each other in so many important respects, and are alike in general features as well as in the mineral composition of their strata and their superficial contents.

On some other occasion, after the Society has heard my account of some other valleys, and seen my measured sections, I shall venture to lay before them the views of the formation of the Somme valley which I hold. I shall now only remark that the bottom of the valley of Saveuse opens into the valley of the Cette, between Montiers and Renancourt, at a height of 92 feet above the sea, and is 140 feet high above the sea near the Ferme de Grâce, and 187 feet above the sea near Saveuse, and that there are continuous terraces of loess, varying in form and elevation, sometimes with the configuration of the chalk-valley on which they repose from one end to the other, but evidently the consequence of water filling the Saveuse valley on its passage from Ferrières to Montiers.

2. *The Gravel and Loess.*—From the fine or coarse character of the gravel, and from the thickness of the loess, we may infer some of the physical circumstances which occurred at the period of deposition. The loess is in some places sandy, and in others is a fine loam, but it varies little in coarseness. At the same height above the river I have observed great discrepancies in the thickness of the true loess: thus, at a pit 200 yards east of the line N Q, 15 feet of loess was well exposed in a new pit from which a good quality of brick-earth was being removed and carried a long distance to the brickfields at Montiers; and I was informed there was a depth of 16 feet more before the gravel was reached. There was a gravel pit on the

same level, a little to the west. On the edge of the Saveuse valley, 400 yards south of O, the loess was only from 1 to 2 feet thick. At St.-Acheul it was only 5 or 6 feet thick; but there were intermediate beds of marl and sand between the true loess and the true gravel there.

There seems to be a line of thicker brick-earth or loess running south and north between the lines O P and N Q. This would indicate that the water was more tranquil at that point. Such differences in currents are very apparent in rivers at the present time; and the warp of our rivers approaches very nearly to the character of loess. The fossiliferous gravels at St.-Acheul extend to a height of 70 feet above the river, or much higher than the corresponding fossiliferous gravels in the valley of the Thames. The shells are found in false-bedded fine sand, and not in clay, at St.-Acheul, and in precisely the same condition as at Crayford.

The Cyrena shell-bed at Crayford, however, is only 38 feet above the sea; but both the St.-Acheul and Crayford gravels extend upward and join the plateau beds, while they pass downward as far as the river in both cases.

The chalk is capped in some places with Tertiary sands at Crayford; but the gravel lies on the concavities of chalk and sand quite indifferently.

The river Cray falls into the Thames much as the river Arve falls into the Somme. The Crayford gravel is 100 feet thick, and confined to a space between two valleys, the eastern valley occupied by the river Cray, and the other and western a dry valley, like that southwest of St.-Acheul.

Bounded by valleys east and west, the Crayford and St.-Acheul gravels lie against escarpments of the chalk parallel to the rivers Thames and Somme.

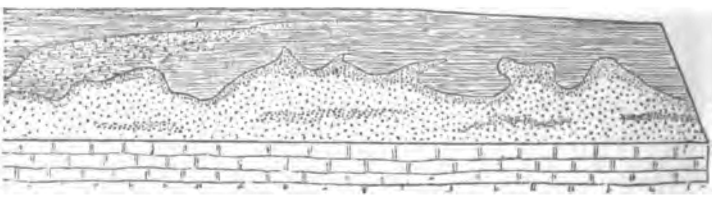
I have not presented more than a few varieties of the gravel sections to be observed in Amiens, for want of room (Plate IV, figs. 12 and 13, and fig. 8 below), and I propose to make some remarks upon the peculiarities of deposition to be observed there at some future time. I will now only observe that the character of the sections, I think, clearly shows us that a large quantity of the gravel material now exposed in the quarries opened for ballast near Amiens had its source in the hills or plateau immediately adjoining and above St.-Acheul and Montiers, and was washed into the valley of the Somme in a direction from south to north, and mingled with the materials brought down by the Somme, flowing from east to west.

The quantity of chalk-detritus is about one-eighth of the whole mass of gravel and loess, and makes the Amiens deposit second in importance to the Brighton gravel, as far as the pres-

ence of chalk is concerned. The unrolled condition in which the large pieces of chalk in the gravel generally occur proves the local origin of the chalk, and that it has been brought down from the high lands and not thrown up by the river.

We might expect an important difference in mineral character between the gravel and loess at the respective heights of 150 and 75 feet above the sea. I have compared the gravel of St.-Acheul, 140 feet above the sea, with that at Montiers, from 70 to 80 feet above the sea, as carefully as I could, in order to find some marked distinctions, but up to the present time without success. I have sketched a piece of gravel at St.-Acheul, 140 feet above the sea (Plate IV, fig. 12), and a piece in La Neuville, 105 feet above the sea, and immediately north (fig. 8).

Fig. 8.—*Section in La Neuville Ballast-pit. Loess and Gravel.*



There is a great deal of variation between these two sections; but there is still more variety in the gravel section of a part of St.-Acheul, 200 yards to the east, at a height of 145 feet above the sea (Plate IV, fig. 13). Similar species of shells have been discovered and named by Mr. Prestwich and others at Montiers and St.-Acheul, at very different levels; but there are none characteristic of any particular elevation above the river. Bones and flint implements are said to be found throughout the Amiens gravels; but as I have never found any myself, nor seen any found, I cannot speak on this point from observation; and it does not appear that these remains, any more than the shells, would enable us to distinguish any particular level.

The large stones of Grès are abundant in all the quarries. I made notes of the numbers and sizes of all I observed, and found that they are also distributed as much in the gravel above the railway as below it, and range up to 4 feet long. There are as many and as large blocks of Grès in the Montiers northern pits as in those at St.-Acheul. I observed one Grès at La Neuville partly covered by loess, the rest of the stone being on gravel; but elsewhere the Grès stones were always in the gravel.

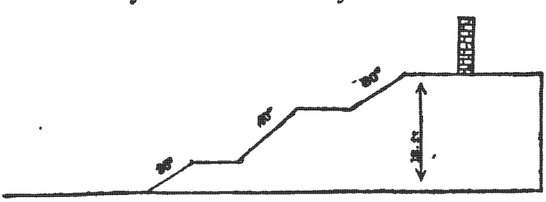
I have mentioned the loess being a very good brick-earth at a point 120 feet above the sea in Montiers. The color and material of the loess is generally a dull brown, varying in propor-

tions of clay and sand and in the amount of angular flints contained in it throughout the whole area. I have, however, remarked a reddish friable brick-earth on the terraces fringing the Somme at Longueau, ninety feet above the sea. This is probably of the same character as that in the similar terrace at Neuville and Montiers. This brick-earth is very similar to that of the river Lea; indeed at Clapton there is a well-marked terrace of brick-earth bounding the marshes, which are composed of gravel. The Clapton terrace is higher than that of the Somme at Amiens, and reposes on London clay, instead of chalk as at Amiens.

This low escarpment of loess is to be seen for a great many miles eastward along the Somme; and, from the angle at which it faces the river, with its flat top, it so resembles a military earthwork that it is often regarded as artificial. I have measured the escarpment at five or six points; and the angles vary from 20° to 40°, the average being 35° (figs. 9 and 10.)

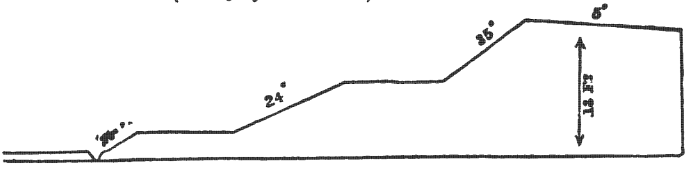
In the Saveuse valley the angles are also various. I have often remarked similar escarpments in England. I made a note

Fig. 9.—*Section near Cagny, in the valley of the Arve. Loess Terrace just above the level of Marsh.*



of a series of terraces, seven in number, one over the other, on the chalk hills, on the north side of the Somme valley, about nine miles from Amiens, on the Paris line; and, indeed, in the space of ten miles you may see twenty small lateral valleys

Fig. 10.—*Section three-quarters of a mile south of M. Dailli's house (valley of the Arve,) Loess Terrace.*



opening into the Somme, with escarpments as distinct and well marked as those drawn of the Saveuse valley.

These were steps cut in the brick-earth of the Saveuse valley by the peasants to enable them to get up the steep sides; but

that was the only information I was enabled to get as to the structure of parts of these terraces, except at Longueau, where a pit was open and good brick-earth visible; so I do not know their relation to the chalk. At Camilla Lucy House, West Humble, near Dorking, I saw a terrace cut into, sloping to the valley at 25°. The gravel was 5 feet thick on the face of chalk, and 7 feet thick 30 yards from the escarpment.

These terraces are of great importance to any one investigating the geology of the Somme, but are not noticed by any other writer as far as I am aware.

V. Conclusion.

What the sections described in this paper plainly tell us is, that the chalk valley of the Somme was excavated exactly to its present form prior to the deposition of any of the gravel now lying in it. Perhaps many layers of gravel may have been deposited and removed again in this ancient chalk valley before the present gravel was deposited; but of this we cannot be certain; so that we must take the first layer of gravel covering the chalk from the higher part of the section to the lower as the oldest in the section, and infer that the remainder of the gravel-series was deposited in regular sequence. The most delicate shells are fossilized in the river-sand of St.-Acheul and Montiers, just as they have been in that of Crayford and Erith.

This is a proof of the peaceful character of the deposition of some part of the Amiens beds, just as the large flints and blocks of Grès, which are so abundant among the gravel, are a proof of the power of the floods which brought the coarse gravel from the plateau, or down the rivers. If the sections near Amiens show the valley-gravel continuous from a height of 200 feet, at St.-Acheul and the Ferme de Grâce, to the river Somme (coated over by a nearly uniform warp of loess), and laid at a low gradient not exactly parallel to the surface of the chalk, but rather in its concavities, then we must necessarily admit that the water of the Somme has at times flowed over the whole surface in question from top to bottom in one flood. This is not an exceptional case at all, as I should have been able to demonstrate, had I been able to bring forward my sections of other river-gravels this evening. We are all agreed that a state of meteorological phenomena existed during the glacial period very different from any now to be met with in these latitudes; therefore there is no *prima facie* improbability in supposing a pluvial period even of longer range in time than the glacial epoch.

The existence of such a pluvial period is demonstrated by the size, constitution, and level of the fluviatile gravel and loess deposits at Amiens and other well-known localities. Large rivers certainly existed to a later date than the glacial period, as they formed such large terraces of loess over the glacial gravels. If we were to judge of the age of these later deposits, such as the loess escarpments at Amiens and Clapton, by their modern appearance and by their being unaltered by weather and not cut into by streams, we should place them almost in the historical period. The Amiens sections of loess accord with those of the Rhine and other rivers. The difference between this loess deposit at Amiens and the present warp of the Somme ought to be an index of the rainfall in the pluvial period, when the loess was deposited, as compared with that falling at the present time; and we may look at these gravels and loess beds as registering rain-gauges.

In the same manner, the comparative rainfall at the epoch of the deposition of gravels might be estimated approximately by comparing the dimensions of the blocks of Grès and large flints moved by fresh water in the gravel-period with the size of the materials moved in the same valleys at the present time.

The existence of a glacial period almost necessitates that of a pluvial period, commencing prior to the glacial, and continuing after it, occupying a region south of that occupied by the ice and snow.

We should have had no cause for surprise if, when the theory of a period of ice and snow in these latitudes was first broached, the probability of a rainy period south of the Thames had been also deduced from a consideration of the effects of so large a mass of ice and snow on the country and atmosphere bordering on the ice-land, but possessing a warmer climate.

We have the evidence in almost all wet valleys of the river merely occupying a small groove cut in the ancient valleys, which valleys I believe, were shaped to their present configuration in such a rainy period as I have inferred. Every wet valley has a number of dry valleys opening into it, which bear the marks of having been shaped by water and continual showers during the pluvial period.

The points of difference between other authors who have written respecting the Somme Valley and myself are as follows :—

In the appendix to Mr. Prestwich's paper in the 'Philosophical Transactions,' M. Pinsard gives the height of the railway at La Neuville as 83 feet above the mean tide at Havre. In the survey made for me by M. Guillom, Principal engineer of

the Chemin de Fer du Nord, the height is 96 feet. (Mr. Prestwich has marked this same level as 76, in his drawing, plate 10. Phil. Trans. 1860.)* This is just 13 feet below the real height. Again, in Mr. Prestwich's section, page 257, Phil. Trans. 1864, the height of the rails at Montiers is marked 130 feet; it should be 99 feet, according to M. Guillom.

It is to be regretted that Mr. Prestwich was supplied with incorrect figures of the relative levels of the ground about Amiens, as the introduction of such errors in the section must have materially affected Mr. Prestwich's theoretical views, as he says, "The upper section at Montiers, which I discovered in 1861, was conclusive as to the relative ages of the gravel" (p. 248, Phil. Trans. 1864.)

In the plan, Plate V, Phil. Trans. 1864, accompanying Mr. Prestwich's memoir, the bare chalk is shown as invariably separating the upper and lower gravels all the way from Amiens to Abbeville; but I have never seen a case of the kind.

It must be remembered that so much gravel has been removed during the last four years, that the sections are now much clearer; and, with the assistance of the accurate measurements of M. Guillom, present examiners have a great advantage over their predecessors, in examining the structure of the gravel near Amiens.

I cannot suppose that Mr. Prestwich would now separate the Montiers gravels, seen in and above the railway-cutting at Montiers, from those in the Great Montiers pit, and into two horizons, as there is only a difference of twenty-two feet between the height of the gravel on the top of the railway-cutting and that in the Imperial road. As nearly the whole space between these two points has now been excavated, the continuity of the gravel is now proved.

When Mr. Prestwich supposed that there was a continuous bare band of chalk separating the gravel in the railway-cutting at Montiers from the gravel near the Imperial road, and that the top of the railway-cutting was (according to the measurement in his section, page 257, Phil. Trans. 1864) sixty feet above the Imperial Road, he very naturally took a different view of the relations of the gravels from what we must take at the present time, with the additional information we possess.

The section on Plate IV, therefore, appears to destroy Mr. Prestwich's argument, on which he has constructed a division of the gravels at St.-Acheul and at Montiers into upper valley-gravels and lower valley-gravels, of different ages, and situated on different horizons, separated, as he supposed, by a band of bare chalk from each other,—the upper valley-gravel being

* This is calculated from the mean tide at St. Valery, which differs 7 feet from that at Havre.

supposed to have been deposited before the excavation of the last fifty feet of the Somme valley, which excavation, he considered, preceded the deposition of the gravels near the Imperial road, Montiers.

The character of the surface of the chalk at Montiers has been discussed at full length in this paper, and shown to be concave at the pits; while it is represented as highly convex at Montiers by Mr. Prestwich and Sir C. Lyell.

In the long section C D (Plate IV, fig. 3,) the St.-Acheul gravel, at a height of 140 feet above the sea, is shown to be separated from the loess at Longueau, at a height of ninety feet, by an escarpment of bare chalk. The tramway (Plate IV, fig. 1), passing from the Imperial road to the railway, crosses one of the supposed bands of chalk marked by Mr. Prestwich. But, instead of chalk, there were nine feet of good gravel and loess exposed in this cutting. The La Neuville ballast-pit, exposing ten feet of gravel and loess, is also on the supposed outcrop of bare chalk (Plate IV, fig. 2: R S on the Plan).

The outline of the sections C D and I K would at first sight seem to confirm the opinions advanced by Mr. Prestwich, that the gravel at the 140-feet level might be of a different age to that 50 feet below it.

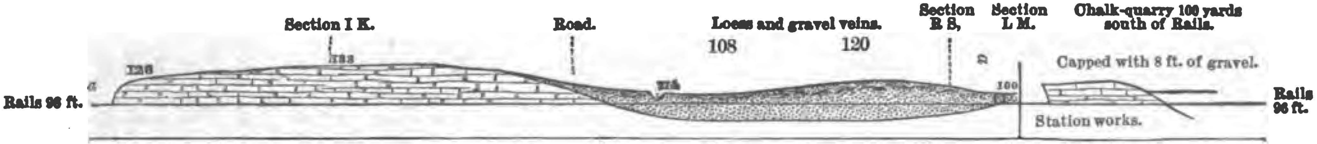
The loess, also, at Longueau, at the 90-feet level, near C, can be traced to La Neuville, and then up to the St.-Acheul pits continuously. The railway-cutting in La Neuville for half a mile is in loess, with veins of gravel (fig. 12); and this is seen to be continuous with the St.-Acheul gravel to the north, by a number of old pits, and in the tramway. The surface of the chalk is concave in this part of the La Neuville valley, between R S and I K, so that gravel and loess would be retained on it; while along the lines C D and I K there is a very steep escarpment, on which no gravel would lie. This escarpment would be swept by the stream of the river Arve and the Somme, flowing at its base, when swollen by large floods, such as must have happened in the gravel-period.

The only inference, I believe, that can be fairly drawn from the sections on Plate IV, is, that all the gravel and loess of St.-Acheul and La Neuville is of one period, and that it remained spread over the valley of the Somme where the ground was concave enough to retain it. The absence of gravel on the steep escarpments and near the river channels is a proof of great floods and rapid currents during the Quaternary period.

I saw the railway-cutting at Montiers, soon after Mr. Prestwich's visit; it was sloped and very much in the state it is at present. (Fig. 12).

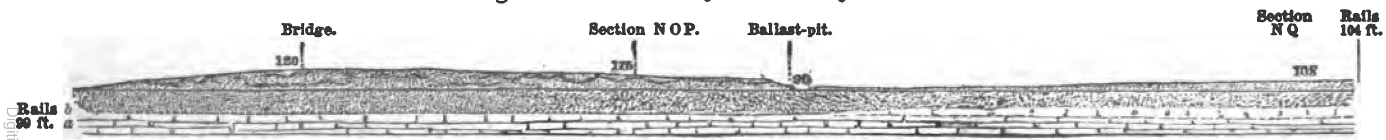
By M. Guillom's section the depth of the chalk below the rails has been accurately determined at two points, where the

Fig. 11.—*Section along the Railway from Longueau, through La Neuville to the Station works.*



Figs. 11 and 12 are only approximative, as this was not included in the part of Amiens kindly surveyed by M. Guillom.

Fig. 12.—*Section along the Railway at Montiers.*



a Chalk. *b* Gravel.

But as the Quaternary beds below the escarpment at Longueau and La Neuville consist only of loess with thin veins of gravel and without fossils, these sections do not support the view taken by Mr. Prestwich that there is a low-level fossiliferous gravel completely separated by an escarpment of chalk from the St.-Acheul gravel.

The veins of gravel in La Neuville thicken toward the west, and the gravel in the La Neuville ballast-pit may continue beneath the Station-works; if so, this gravel is part of the stream of gravel which has descended from St.-Acheul down the La Neuville valley into the Somme, and can be traced from St.-Acheul continuously to the La Neuville ballast-pit, and passes (in thickness unknown) under the rails, as represented in the Section, fig. 11. The La Neuville deposits are not really separated from St.-Acheul by a bare band of chalk, but they wrap round the base of the high stratum of chalk which is seen in L M and I K, and which forms the east and west side of the La Neuville valley.

sections N Q and N O P cross the railway (see figs. 8 and 9, Plate IV). In fig. 8, M. Guillom found the chalk 8 feet below the rails; in fig. 9, 14 feet below the rails. Mr. Prestwich has represented this railway-cutting as on one of his bands of chalk, dividing the valley-gravels into two horizons; and in consequence, I had the section N O P taken, as nearly as I could, on the same line as Mr. Prestwich, because I had always remarked gravel in the Montiers railway-cutting, and not chalk. I also give a section along this railway (see fig. 12, page 324). By the French survey, the chalk is 14 feet below the rails. In Mr. Prestwich's section of the same place, it is 20 feet above the rails. This difference of 34 feet added to the error in the height of rails, before mentioned, of 31 feet, makes a total difference of 65 feet in the height of the chalk between Mr. Prestwich and M. Guillom, supposing I am correct in placing Mr. Prestwich's section on the line N O P.

Mr. Prestwich has recently informed me that he considers his section was intermediate between the lines N O P and N Q. As the railway-cutting ceases at the ballast-pit (fig. 12,) and there is an embankment to the west of that point for some distance, it is difficult to place Mr. Prestwich's section at any other point than where I supposed it was taken, on account of the configuration of the ground. Whether there was chalk, or not, at any one point, is quite immaterial to my argument. I do not find the Montiers section at all as represented by Mr. Prestwich and Sir C. Lyell. (See fig. 12, p. 324.)

The Montiers section appears to be the one adopted as a type of the Somme district, first by Mr. Prestwich and afterward by Sir C. Lyell. Both authors represent, in several sections of the Somme, a great extent of chalk, separating highly inclined beds of gravel, which they have distinguished in age by its position above or below this outcrop of chalk, as upper- and lower- or high- and low-level gravels. The sections which I place before the Society appear to me, on the contrary, to show that this distinction is not a real one, but that the deposit of gravel is one and continuous, deposited in concavities of an ancient chalk valley, and is not highly inclined as represented in the 'Antiquity of Man' and the 'Philosophical Transactions.'

In page 264, Phil. Trans. 1864, Mr. Prestwich gives a theoretical account of the view he takes of the deposition of the gravels. Part of the upper-level gravels are represented as remaining untouched, while the valley is cut down 50 feet, and a newer set of gravels deposited at lower levels; my sections show that there is no evidence of any such action.

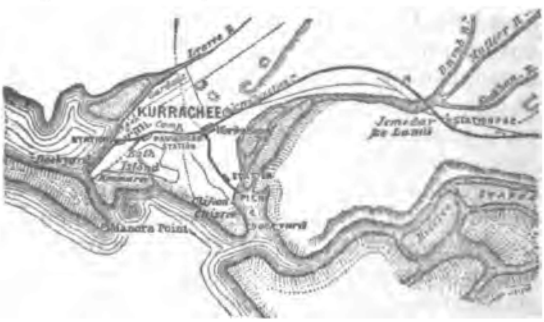
The same views are extended by Mr. Prestwich to the loess deposit; the loess of St.-Acheul is considered a much older deposit than the loess at Montiers.

Mr. Prestwich lays great stress, in his paper in the Society's Journal, p. 500, on the valley being too large to admit of the possibility of its being filled with water up to a height of 100 feet above the present water-level.

I have already submitted the argument that we ought to judge of the height of a flood by means of the *debris* it has left, and not by any theoretical notions of our own.

In 1866, twenty inches of rain fell in Scinde in twenty-four hours, in a flat country intersected by rivers. Nine girders, weighing nearly eighty tons each, were washed off the piers by the Mulleer river from the Railway bridge, situated sixteen miles above Kurrachee (fig. 13). This bridge consisted of eighteen girders (see Plate IV, fig. 10). They were not box girders, but made of wrought iron on Warren's system. The bottoms of these girders were sixty-five feet above high-water mark, spring tides, Kurrachee harbor, and seventy-four feet above low-water spring tides. They fell in the course of six hours; and one girder of the weight of eighty tons was carried two miles down the river and nearly buried in sand.* The section of the river bridge is represented (Plate IV, fig 10). The fall of the Mulleer river only averaged ten feet per mile for fifteen miles above the bridge; and as rain rarely falls, there is generally less than a foot of water in the river-bed. This bed was nearly dry

Fig. 13.—*Map of the district near Kurrachee.*



the day after, as well as the day before this excessive rainfall. Other instances of the same kind have been reported from India. The river first banked up wood and grass against the bridge and then threw the girders over. The weight of these girders moved in a river-bed of the dimensions given (Plate IV, fig. 11)† is an index of the rainfall in Scinde, just as the

* Mr. John Brunton, F.G.S., Chief Engineer of the Scinde Railway, was present at the Meeting, and confirmed this statement, which he had previously given me.

† Figs. 10 and 11, Plate IV, are from drawings supplied by Mr. W. A. Brunton, C. E.

fluviatile beds at Amiens are an index of the current of the Somme, of its flood-level, and the force of its stream. We cannot determine the rainfall at Amiens in the Quaternary Period except by its results in the form of gravel-deposits; and these appear to give as good indications as the fall of the Mulleer bridge girders does of the flood in that river.

ART. XXXI.—*On the Artificial Formation of Organic Substances;* by C. GREVILLE WILLIAMS, Esq., F.R.S.*

CHEMICAL researches are liable at various epochs to take special directions. Before 1830 organic chemistry was comparatively little studied. The simplification of the methods of organic analysis by Liebig took place at a most opportune moment, and gave an extraordinary impetus to the study of carbon compounds. So great was this influence that proximate and ultimate analysis made a progress, the rapidity of which was unexampled in the history of science.

But chemists soon became dissatisfied with merely determining the composition of substances, and they very soon began eagerly to study their products of decomposition, and in this manner get a clue to the way in which nature had put them together.

The successful attack on this problem led to a much grander one suggesting itself. This was to utilize the insight analysis had given them into the constitution of substances, and to endeavor to build them up without the assistance of life. The speaker showed that we thus arrive at the two great engines of chemical research, *analysis* and *synthesis.*

He then proceeded to define and illustrate experimentally these terms.

In organic chemistry the information supplied by the analysis of a substance often renders its synthesis easy. Water was decomposed by a battery, and its properties and quantitative relations shown. The mixed gases were then introduced into a soap-bubble, so prepared as to last a considerable time. It was by a simple contrivance attached to a thread, and the lightness of the enclosed gases was shown by the fact that the bubble was able to raise the thread and a disc of paper into the air. The energy with which the mixed gases combine to form water was then shown by applying a light to the bubble, when it burst with a loud report. The *quantitative* synthesis of water was experimentally shown by passing hydrogen over cupric oxyd in an apparatus which allowed of the collection of the water. It

* From the Proceedings of the Royal Institution of Great Britain, May 8, 1868.

was then shown that in organic chemistry the molecules are too complex to be put together so easily; and this statement was proved by reference to the constitution of methylamine, the simplest of the organic alkaloids.

The speaker then went somewhat fully into the question of the propriety of the use of the terms "organic" and "inorganic." He shewed also that all the attempts hitherto made at separating chemistry into two distinct branches had failed. Liebig's definition of organic chemistry as the "chemistry of compound radicals" being obviously inadequate, inasmuch as some compound radicals (such as sulphuryl and phosphoryl) are certainly inorganic.

Laurent's definition, "chemistry of carbon," is equally insufficient, inasmuch as carbonic anhydrid and carbonic tetrachlorid are as clearly inorganic as sulphuric anhydrid or sodic chlorid. He then proceeded to argue that chemistry was "one and indivisible," and stated that one of the chief aims of his discourse was to prove that assertion.

It was shown that until within the last few years, all the specific attempts made to break the apparently natural barriers between organic and inorganic chemistry had proved failures.

It was true that in the course of innumerable researches and experiments made by chemists, one or two of the simple organic bodies had presented themselves; but, like urea and cyanogen, they were substances which, as it were, hovered on the confines of inorganic chemistry, and would have been called inorganic had they not contained carbon.

The grand problem, which consisted in taking the elements themselves, and building them up *gradatim* into the proximate principles existing in the tissues of plants and animals, until lately appeared almost hopeless. This apparent difficulty was shown to arise from the mistake of supposing the proximate principles of animals and vegetables to result from an occult power vaguely termed the "vital force." It was at one time supposed that the laws which regulate combination were either suspended or modified in the tissues of living creatures, but the speaker urged that whenever the proper reagents were made to act upon each other under the proper conditions, the same substances were produced which at one time were supposed to require the aid of vitality for their formation.

The problem of the "synthesis," or building up of the so-called organic substances, was then shown to present itself (in the present state of chemistry) under two aspects :—1st. Where they are prepared by the aid of reagents, which have themselves been produced directly or indirectly from animals or vegetables. 2nd. Where the synthesis was effected from the free elements themselves, from hydrogen and pure carbon.

The speaker then proceeded to enumerate some of the principal instances where substances originally derived from animals or vegetables had been formed synthetically. Wöhler's synthesis of urea was shown to be one of the earliest in point of date, and his method was described, and also Kolbe's new process by the mere heating of ammonic carbonate to a point just below that at which urea is decomposed.

One of the next most important steps in the history of synthesis was shown to be the conversion of carbonic disulphid into carbonic tetrachlorid or perchlorinated marsh gas. Inasmuch as carbonic disulphid is a purely inorganic body, it is evident that any substance which can be formed from it is a case of true synthesis.

Ths following equations represent the steps by which acetic acid may be produced from carbonic disulphid :—*

$$\underbrace{CS_2}_{\text{Carbonic disulphid.}} + 6Cl_2 = \underbrace{CCl_4}_{\text{Carbonic tetrachlorid.}} + \underbrace{2(SCl_4)}_{\text{Sulphuric tetrachlorid.}}$$

$$\underbrace{2(CCl_4)}_{\text{Carbonic tetrachlorid.}} = \underbrace{C_2Cl_4}_{\text{Carbonic dichlorid.}} + 2Cl_2$$

$$\underbrace{C_2Cl_4}_{\text{Carbonic dichlorid.}} + Cl_2 = \underbrace{C_2Cl_6}_{\text{Carbonic trichlorid.}}$$

$$\underbrace{C_2Cl_6}_{\text{Carbonic trichlorid.}} + 2(H_2O) = \underbrace{C_2HCl_3O_2}_{\text{Trichloracetic acid.}} + 3(HCl)$$

$$\underbrace{C_2HCl_3O_2}_{\text{Trichloracetic acid.}} + 3H_2 = \underbrace{C_2H_4O_2}_{\text{Acetic acid.}} + 3HCl$$

This important series of reactions, then, result in the production of acetic acid, one of the most marked of the so-called organic acids, from purely inorganic materials.

The synthesis of oxalic acid by the direct union of carbonic anhydrid with sodium, as recently accomplished by Dr. Drechsel, was next described, and it was shown that as oxalic acid, by mere distillation, yields formic acid, the synthesis of the first acid leads directly to a new synthesis of the second.

The other modes of effecting the synthesis of formic acid were then pointed out, viz :—Berthelot's process, which consist in heating potassic hydrate in an atmosphere of carbonic oxyd; and Kolbe and Schmidt's methods, by exposing potassium to a warm moist atmosphere of carbonic anhydrid.

The speaker, in the course of his remarks on the constitution of formic acid, showed that the quantity of oxygen in it was so large that it only required one atom more to convert it into carbonic acid and water. Its easy oxydation was illustrated by

[* C=12, O=16, S=32, etc.]

letting it fall on plumbic dioxyd in an apparatus which caused the evolved gas to pass into a solution of baric hydrate, the result being a copious precipitation of baric carbonate.

Having shown that acetic acid can be formed from carbonic disulphid and the chlorids of carbon, and oxalic and formic acids from the oxyds of carbon, the speaker proceeded to indicate the modes in which complex bodies, hitherto obtained from animal and vegetable sources, can be built up from elemental carbon and hydrogen.

If carbon can only be made to combine directly with hydrogen, no matter how simple the resulting compound may be, it becomes possible to effect the synthesis of a vast number of the most characteristic substances found in animals and vegetables.

This brilliant result has been accomplished through the agency of acetylene, a most remarkable hydrocarbon which was first noticed by Edmund Davy as long ago as 1836.

There are two methods by which acetylene can be formed from inorganic materials—one devised by Berthelot, and the other by the speaker. The first consists in passing a stream of hydrogen through a globe in which the voltaic arc (from 70 or 80 cells of a Grove's battery) is produced between carbon points. At this tremendous temperature the carbon unites directly with the hydrogen. The experiment was made, and the production of acetylene shown, by the formation of a precipitate in a solution of ammoniacal cuprous chlorid.

The speaker then showed, experimentally, that much larger quantities of acetylene can be formed by the decomposition by the induction spark of carbonic tetrachlorid in presence of hydrogen, in accordance with the equation:—

$$2(CCl_4) + 5H_2 = C_2H_2 + 8(HCl).$$

The experiment succeeded perfectly, and a large quantity of the cuprous acetylid was rapidly produced.

But the most simple and ready means of preparing acetylene was shown to be by drawing air through the flame of a common glass spirit-lamp, by means of an aspirator. So readily is the cuprous precipitate obtained by this means, that it suffices to draw a few cubic centimeters through the solution of ammoniacal cuprous chlorid to obtain evidence of the presence of acetylene in the flame. The experiment was then made, and in a few seconds the solution became thick with the suspended precipitate.

The speaker had ascertained that all the homologues of olefiant gas give acetylene in abundance when subjected to the induction spark. Amylene does it readily in accordance with the annexed equation:—

$$\underbrace{2(C_5H_{10})}_{\text{Amylene.}} = \underbrace{5(C_2H_2)}_{\text{Acetylene.}} + 5H_2.$$

That the spark acted only in consequence of its high temperature, the speaker said, is rendered probable by the fact that if hydrogen be passed through carbonic tetrachlorid, and then into a globe containing a platinum spiral, when the latter was heated to dull redness by three cells of a Grove's battery, no acetylene was produced; but when five cells were used, and the spiral became white hot, the cuprous precipitate was obtained readily. The same result was stated to occur with amylene.

Simple as the formula of acetylene is, almost all the animal and vegetable substances which have been formed hy pure synthesis may be obtained from it.

The following equations represent the steps by which alcohol may be "synthesised." It is proper to premise that the conversion of acetylene into olefiant gas is accomplished by treating the cuprous acetylid with zinc and ammonia, so as to obtain nascent hydrogen :—

$$\underbrace{C_2H_2}_{\text{Acetylene.}} + H_2 = \underbrace{C_2H_4}_{\text{Ethylene.}}$$

$$\underbrace{C_2H_4}_{\text{Ethylene.}} + H_2SO_4 = \underbrace{H, C_2H_5SO_4}_{\text{Ethyl-sulphuric acid.}}$$

$$\underbrace{H, C_2H_5SO_4}_{\text{Ethyl-sulphuric acid.}} + H_2O = \underbrace{C_2H_6O}_{\text{Alcohol.}} + H_2SO_4$$

The synthesis of succinic acid from acetylene was next shown in accordance with the annexed equations, omitting the synthesis of ethylene, which has been already given:—

$$\underbrace{C_2H_4}_{\text{Ethylene.}} + Br_2 = \underbrace{C_2H_4Br_2}_{\text{Ethylenic dibromid.}}$$

$$\underbrace{C_2H_4Br_2}_{\substack{\text{Ethylenic}\\ \text{dibromid.}}} + \underbrace{2(KCN)}_{\substack{\text{Potassic}\\ \text{cyanid.}}} = \underbrace{C_2H_4(CN)_2}_{\substack{\text{Ethylenic}\\ \text{dicyanid.}}} + 2(KBr)$$

$$\underbrace{C_2H_4(CN)_2}_{\text{Ethylenic dicyanid.}} + 2(KHO) + 2(H_2O) = \underbrace{C_4H_4K_2O_4}_{\text{Potassic succinate.}} + 2(NH)_3$$

This mode of effecting the synthesis of succinic acid is due to the researches of Maxwell Simpson.

The beautiful appearance of succinic acid under the influence of polarized light was shown by the aid of the electric lamp. The specimen used had been artificially prepared.

The speaker then proceeded to show that the synthesis of succinic acid was a direct step to that of tartaric acid. This latter reaction is due to the researches of Perkin and Duppa.

The artificial formation of succinic acid, starting with acetylene, having been proved, it is only necessary to start with that acid to prove the synthesis of tartaric acid from acetylene:—

$$\underbrace{C_4H_6O_4}_{\text{Succinic acid.}} + 2Br_2 = \underbrace{C_4H_4Br_2O_4}_{\text{Dibromosuccinic acid.}} + 2HBr$$

$$\underbrace{C_4H_2Ag_2Br_2O_4}_{\text{Argentic dibromosuccinate.}} + 2(H_2O) = \underbrace{C_4H_6O_6}_{\text{Tartaric acid.}} + 2(AgBr)$$

The speaker then proceeded to show how the synthesis of the organic alkaloids could be effected from inorganic materials.

In the first place the fact that cyanids can be produced by heating carbon in presence of nitrogen and an alkali, is well known. The next step is to procure prussic acid by distilling cyanids with acids. From pure prussic acid, methylamine, the simplest of the primary monamines, can easily be obtained, either by the aid of nascent hydrogen, or free hydrogen in the presence of spongy platinum.

$$\underbrace{CHN}_{\text{Prussic acid.}} + 2H_2 = \underbrace{CH_5N}_{\text{Methylamine.}}$$

This equation has been realized by both the methods given above, the first by Mendius, the second by Debus.

It is also evident that as alcohol can be obtained from acetylene, that all the ethylic, primary, secondary, and tertiary monamines of Hofmann can now be synthetically formed. The steps being, (1) conversion of acetylene into olefiant gas; (2) passage of olefiant gas into alcohol; (3) alcohol into iodid of ethyl; (4) action of iodid of ethyl on ammonia.

Again, acetic acid it has been shown, can be prepared from carbonic disulphid. Now acetic acid, by the action of a red heat, can be made to yield a number of the homologues of olefiant gas. The latter by treatment with excessively strong hydriodic acid, become converted into the iodids of the alcohol radicals (Berthelot). By following up this last reaction with pentylene, heptylene, octylene, and nonylene, the speaker succeeded in obtaining pentylamine, heptylamine, octylamine, and nonylamine.

The direct ascent from acetylene to the coal tar colors was then shown according to the following equations:—

$$\underbrace{3(C_2H_2)}_{\text{Acetylene.}} = \underbrace{C_6H_6}_{\text{Benzol.}}$$

$$\underbrace{C_6H_6}_{\text{Benzol.}} + HNO_3 = \underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + H_2O$$

$$\underbrace{C_6H_5NO_2}_{\text{Nitrobenzol.}} + 6(FeO) + H_2O = \underbrace{C_6H_7N}_{\text{Aniline.}} + 3(Fe_2O_3)$$

$$\underbrace{C_6H_5Br}_{\text{Bromobenzol.}} + \underbrace{CH_3Br}_{\text{Methylic Bromid.}} + Na_2 = \underbrace{C_7H_8}_{\text{Toluol.}} + 2NaBr$$

$$\underbrace{C_6H_7N}_{\text{Aniline.}} + \underbrace{2(C_7H_9N)}_{\text{Toluidine.}} = \underbrace{C_{20}H_{19}N_3}_{\text{Rosaniline.}} + 3H_2$$

These transformations were all described at length. In effect acetylene passed through a red hot tube becomes polymerized into benzol.

The passage of toluol into nitro-toluol and toluidine is omitted in the above equations, because the reactions are identical in kind with those of benzol into aniline.

In describing benzol, experiments were shown illustrating the density of its vapor as compared with air. In one of these, benzol was poured into a large beaker containing a hot iron; at first the benzol assumed the spheroidal form, but, as the temperature fell, it became converted into vapor, which filled the beaker. A glass syphon was then introduced into the beaker, and the vapor drawn off as if it had been a liquid, and inflamed. The vapor descending through the syphon was then received into a warm beaker, from which it was decanted into another beaker in which it was inflamed.

The speaker then proceeded to show the way in which the synthesis of zinc-ethyl could be effected; it is, however, unnecessary to follow the equations in detail, because, having already explained the manner in which alcohol can be synthesised from acetylene, it is obvious that zinc-ethyl can be directly derived from that fluid.

Wanklyn's interesting synthesis of acetic acid from sodium-methyl was then shown to take place in accordance with the expression:

$$\underbrace{CH_3Na}_{\text{Sodium-methyl.}}+CO_2=\underbrace{C_2H_3NaO_2}_{\text{Sodic acetate.}}$$

The method appears to be general, inasmuch as the same chemist has effected the synthesis of propionic acid:

$$\underbrace{C_2H_5Na}_{\text{Sodium-ethyl.}}+CO_2=\underbrace{C_3H_5NaO_2}_{\text{Sodic propionate.}}$$

And substituting carbonic oxyd for carbonic anhydrid, we have—

$$2(CH_3Na)+CO=\underbrace{C_3H_6O}_{\text{Acetone.}}+2Na$$

The speaker stated that one of the most interesting of the cases of synthesis recently accomplished was that in which Mr. W. H. Perkin had succeeded in producing artificially the odoriferous principle of new hay and the tonquin bean.

The delicious fragrance of new hay is entirely due to the presence of the sweet-scented vernal grass, *anthoxanthum odoratum.* It is the same substance which is the cause of the sweet smell of the woodruff, *asperula odorata ;* and the melilot, *melilotos officinalis.* It is also the flavoring ingredient in the *mai wein* of the Germans, which is perfumed with woodruff.

Until lately, nothing was known about coumarin, except that it was a colorless crystalline body, having the formula—

$$C_9H_6O_2.$$

The crystals of coumarin appear very beautiful under the influence of polarized light. The image of some artificial coumarin, which had been fused and allowed to crystallize in a plate of glass, was then thrown upon the screen, and the light being polarized by the aid of Nicol's prisms, the crystals assumed the most gorgeous and varying colors as the prisms were rotated.

The clue to its constitution was shown to be the circumstance that when heated with potassic hydrate it yields salicylic and acetic acids. The production of salicylic acid from coumarin was then shown experimentally, the presence of the acid being proved by its yielding a deep purple coloration with ferric chlorid.

Artificial coumarin was obtained from the hydrid of salicyl. By treatment with sodium it yielded hydrid of sodium-salicyl; this substance, heated with acetic anhydrid, gave hydrid of aceto-salicyl. This last substance was then distilled with acetic anhydrid and sodic acetate, and when the temperature reached 290°, the distillate solidified to a mass of crystals of pure coumarin, having all the fragrance and beauty of that obtained from the tonquin bean.

The speaker then submitted that the assertion he had made at an early period of his discourse that there was no natural barrier between organic and inorganic chemistry, had been amply proved by the instances he had brought forward. He said that they had studied together that evening several cases where, starting from inorganic matter, they had ascended step by step until they had reached some of the most complicated bodies secreted by animals and vegetables. What, he said, could be more distinctly inorganic than nitrogen, carbon, and oxygen ? What more distinctly an animal secretion

than urea? What more completely inorganic than acetylene? What more distinctly vegetable in origin than coumarin?

Chemists have then, so far, done what a very few years ago would have been regarded as possible only by aid of the vital force. A true organized substance, he said, is so definite that we can almost invariably determine its molecular weight, and it is generally crystalline. But when we come to the tissues we are dealing no longer with organic substances, but with organized beings, and feel that we are approaching the barriers which separate the study of life from the study of matter. The bonds which unite them are so close that we cannot imagine life *without* matter, and it is equally difficult to conceive the assumption of vitality *by* matter; but we must never cease to look anxiously for the solution of the problem. The impossible is a horizon which recedes as we advance, and the *terra incognita* of to-day will to-morrow be boldly mapped upon every schoolboy's chart!

Art. XXXII.—*Notes on the Caucasus;* by Capt. F. von Koschkull. (Communication addressed to Prof. J. S. Newberry, and translated by him for this Journal.)

(Continued from page 222.)

Aside from the ores which have been mentioned and which are associated with the igneous and crystalline rocks, the sedimentary formations also contain minerals of value.

The inferior strata of the Jurassic formation include important beds of coal, which are found on both slopes of the main chain of the Caucasus, the richest deposits being in the northwestern portion of the belt between Mt. Elbruz and Mt. Tisent. In the northern part of this area, coal has been discovered in the valleys of the Kuban and its affluents, Maraukle, Zelentschuk, Urup, Laba and Belaya. During the last ten years coal has been mined on the Kuban to the amount of about 4,000,000 kilograms per annum.

On the southern slope of the mountains, the beds of coal crop out on the banks of the upper Rion and its tributaries from the north, and have an aggregate thickness of about 28 feet. Except an insignificant quantity of a peculiar quality of coal, of which the inhabitants make ornaments, rosaries, buttons, brooches, etc., the coal of the Rion is not yet mined, the abundance of wood in that region fully supplying the present want of fuel; but with the construction of the proposed railroad from the Black sea to the Caspian these rich deposits will undoubtedly be brought into use.

The Jurassic strata of the Caucasus also contain rich deposits of sulphur. These are found in the eastern part of the mountain belt on the river Sulak. During the war with Russia the mountaineers obtained from this locality sulphur for the manufacture of gunpowder; but since their submission the mines have been entirely unworked.

Salt, alum, petroleum and asphaltum are met with in the Tertiary formation. The salt occurs in two forms, viz: that of rock salt, of which thick and wide-spread strata have been found, and that dissolved in or deposited from the water of salt lakes. The principal beds of rock salt are in the valley of the Araxis, both east and west of Mt. Ararat, and geologically are lower Tertiary. These deposits of rock salt have evidently been known and worked from the most remote period; as among the modern mines are numerous and extensive ancient workings of which no historical record exists.

In 1865, when engaged in a survey of the salt district, my attention was attracted by some of these old mines, which were regarded by the inhabitants as natural caves.

I soon satisfied myself, however, that they were the work of man, and resolved to explore them. My guides would fain have dissuaded me from carrying the plan into execution, as their imagination had peopled all these subterranean passages with evil spirits, of which they stood in great fear. Disregarding their superstitions, I penetrated one of the ancient adits, and partially explored what proved to be a very extensive and fairly well wrought mine, evidently of great antiquity. At the remote end of some of the galleries I found heaps of mined salt and hundreds of tools of various forms and sizes, either perfect or more or less worn and broken. These implements were for the most part picks or pick and hammer combined, varying from 5 to 15 inches in length, wrought with great labor and considerable skill out of a tough hornblendic rock. None of these tools were pierced for the insertion of handles, but all were encircled by grooves for the reception of withes or thongs. In this respect, as in the character of the material of which they were formed, resembling many of the stone implements of Europe, and being apparently the product of the same age, or more accurately speaking, of the same stage of intellectual development.

The salt lakes of which I have spoken occur on the peninsulas of Taman and Apscheron, and on the plains bordering the rivers Terek and Kuban. From the concentration of the saline solutions of these lakes in summer, on many of them a crust of salt forms, which attains a thickness of from three to six inches. This crust is collected by the inhabitants for use.

The total production of salt in the Caucasus is about 24,000,000 kilograms of rock-salt, and 16,000,000 kilograms of lake-salt per annum.

Aside from the salt lakes to which I have referred, there are others in the valley of the Kur and Araxes, of which the waters contain large quantities of sulphate of magnesia and carbonate of soda.

Alum stone, which is very rich in alum, occurs on the northern slope of the Little Caucasus, and there is produced from it a yearly product of about 16,000 kilograms of alum.

Of the minerals of the Tertiary formation, by far the most important are petroleum and asphaltum. All the accumulations of these substances are in the upper Tertiary strata, and are distributed geographically in several districts, of which the most important are the extremities of the great Caucasian chain, which form the peninsulas of Taman and Abscheron; on the flanks of the mountains on the north, in the valleys of the tributaries of the Kuban, and of the river Saundga (Sundscha); on the southern slope, in the valleys of the Rion and Kur.

A considerable degree of system is noticeable in the distribution of the springs of petroleum in each of these districts, and they are found to bear certain relations to the principal lines of upheaval which traverse them. Sometimes the springs issue from the summits of the anticlinal axes, and sometimes in parallel lines along the slopes of the upturned strata. The petroleum of the Caucasus consists of two well marked varieties; one, very fluid, of a light yellow color, found only on the peninsula of Abscheron; the other, dark brown in color, varying in density from that which is very thin to mineral tar and asphaltum. Carburetted hydrogen gas issues from all the petroleum springs, and is most abundant where the petroleum is lightest.

The oil springs of the Caucasus have been known and the oil collected from them for ages. Upon the peninsula of Abscheron, and on the banks of the Saundga and Kur, the petroleum is collected in pits of greater or less depth excavated in the sandy strata of the Tertiary age. The deepest of these pits descend from 70 to 100 feet from the surface, while the shallowest are only two feet.

In these pits the oil is accompanied by water, from which it is dexterously separated by skimming. The instrument by which this is effected is a sack of green hide, the mouth encircled by an iron ring, the sack being suspended by a cord.

As a general rule the wells are skimmed once a day. The quantity of petroleum obtained from the different pits is very variable, but is nearly constant for the same pit. The opening

of new wells in the vicinity of old ones seems not to affect the yield of the latter, so that the productiveness of a given area depends directly on the number of wells which it contains. This fact has determined the method pursued in the collection of petroleum on the island of Tschelaken and on the eastern shore of the Caspian, where to obtain a large quantity of oil 20,000 wells have been sunk, but none to any great depth. The Persians, who were the first to collect the oil of the peninsula of Abscheron, seem to have followed the same system of exploitation.

The number of wells dug there is very great, and they are very closely approximated. In 1863, a new well was sunk by the side of one which had for centuries yielded about 1,600 kilograms of petroleum per day. The new one now produces 19,200 kilograms every twenty-four hours without affecting in the least the yield of the old well.

In 1865, the method pursued with so much success in America in the exploitation of petroleum was introduced into the Caucasus and with complete success. In a locality selected by myself, near some oil springs in the valley of one of the tributaries of the Kuban, a well was commenced in the above mentioned year. At the depth of 40 feet encouraging signs were met with, and in Feb., 1866, when the boring had reached a depth of 242 feet, the petroleum burst out in a jet which was maintained at a height of 42 feet until controlled.

At first the production of this fountain well was 32,000 kilograms per day, but the yield gradually diminished to about one-half that amount. There have since been periods when the flow was entirely arrested, but apparently in consequence of the clogging of the well, as when cleaned out, the normal flow was resumed.

During 57 days in 1866 during which the flow from this well was carefully measured, the production was 880,000 kilograms of petroleum, with one-tenth of that quantity of water; and during 196 days of service this well has produced 2,160,000 kilograms of oil.

The temperature of the oil as it flows from this well is 7° Reaumur, that is to say, 3° less than it should be according to the geothermic law for a liquid drawn from a depth of 242 feet below the surface. Can this depression of temperature be attributed to the absorption of heat in the conversion of a portion of the petroleum into gas?

I learn by a letter recently received from the Caucasus that a new well sunk in the vicinity of that just described, having reached in November last the depth of 258 feet, opened a fissure from which the oil issued with such force as to form a jet 60 feet in height, and yielding 64,800 kilograms of oil per day.

In addition to the petroleum produced by the wells and springs in the valley of the Kuban, of which no accurate measurement has yet been made, the total annual yield of the oil region of the Caucasus may be estimated as follows:

		Kilograms.
1.	Petroleum produced on the peninsula of Abscheron,	8,640,000
2.	" " in the valley of the Kur,	192,000
3.	" " " " " Saundga,	320,000

In all the oil districts of the Caucasus in the vicinity of the oil springs, deposits of asphaltum and ozokerite have been discovered. Not long since a beginning was made in the use of these substances for the manufacture of paraffine, and the amount now obtained from this source is 100,000 kilograms per annum.

No sketch of the physical geography of the Isthmus of the Caucasus can be considered complete which does not include some notice of the action of subterranean forces which even now are producing many striking phenomena. The continued activity of the causes which in former periods so completely changed the topography of this region, is manifested by frequent earthquakes, by eruptions of mud volcanoes, and by numerous thermal springs.

As regards the earthquakes, the Isthmus of the Caucasus may be divided into several sections, each of which has its center of action. On the northern slope of the great Caucasian range, one of these shaken districts lies north of Mt. Elbruz, including the area drained by the tributaries of the rivers Kuban, Kuma and Malka, with its center near Pjatigorsk. The other northern district includes all the space on the south-east of the river Terek, with its center at Grognaga. On the south side of the Great Caucasus one district is formed by the lower valley of the Kur, with its center near the city of Schemakha. The second district includes the middle portion of the valley of the Kur, with its center at Tiflis; while the third district comprises the northern part of Persia and Asia Minor, having its center in Mt. Ararat. Earthquakes are so common in the Caucasus that they may be almost said, like the sunrise, to form part of the daily experience of the inhabitants.

The severity of these earthquakes varies greatly; some are terribly destructive, as was that which a few years since destroyed the fortress of Nazrau on the north side of the Caucasus, and that other which nearly demolished the city of Schemakha.

The mud volcanoes now, or recently in action, are for the most part confined to the extremities of the great Caucasian chain, that is, to the peninsulas of Abscheron and Taman.

These mud volcanoes generally form conical hills from 200 to 1,000 feet in height, on the summits of which are craters with many cones of eruption. The diameter of the craters is sometimes considerable (as much as 700 feet).

The ordinary and moderate action of these volcanoes is sometimes interrupted by paroxysms of great violence, attended by copious eruptions and by earthquakes. The phenomena displayed by one of these volcanoes when in vigorous action was first described by the famous naturalist and traveler, Pallas, in 1793. This was situated on the peninsula of Taman, where precisely similar exhibitions may be now observed. New cones are also produced now, as formerly, and it has been observed that they are formed along certain well defined lines which correspond accurately with the principal axes of elevation which traverse the Isthmus.

On the peninsula of Taman the ejections from the mud volcanoes are found covering many of the mounds which contain the tombs of the ancient Greek colonists who inhabited this region. At the other extremity of the Caucasus in May, 1861, an island was formed by volcanic eruptions in the Caspian Sea, between the peninsula of Abscheron and the mouth of the Kur. In the course of a few months this island was washed away by the waves.

The thermal springs to which I have alluded, are for the most part confined to the flanks of the main chain and of the Little Caucasus. Yet they are sometimes found in the mountains at a considerable elevation. All these springs form certain groups disposed in lines parallel with the great axes of elevation. The water of the hot springs sometimes reaches the temperature of 72° Reaumur, but in this respect as well as in regard to the matters held in solution, there are marked differences between the different springs even when closely approximated.

The most remarkable groups of springs are on the northern side of the principal chain of mountains. 1st. Those north of Mt. Elbruz, near the city of Pjatigorsk, interesting from the number of the springs, and the peculiarity and diversity of their waters. 2nd. On the river Soundja. These are sulphur springs, and some of them very hot (72° Reaumur). 3rd. Along the river Saulak, also sulphurous, with a temperature of 42° Reaumur. 4th. On the Kuban; like the last sulphur springs, and having the same temperature (42° Reaumur).

South of the Great Caucasian chain are three groups of hot springs, one in the city of Tiflis, sulphurous water, with a temperature of 30° Reaumar; another near the city of Schemakha; the third in the mountains of Karthlo-Imeritia; this

latter group is remarkable for the diverisity of character exhibited by the different springs.

Even in this brief description of the Isthmus of the Caucasus, it seems necessary that something should be said of the relations which exist between the orography and the climate, population and productions.

The principal chain of the Caucasus divides the country into two districts, which present marked differences of climate.

The plain drained by the rivers Terek and Kuban passes imperceptibly on the north into the steppes of Southern Russia. The southeastern portion of this plain bordering on the Caspian Sea is represented by broad sandy surfaces and extensive "salt-pans;" while the interior and western portions consist of low prairies, marshes and mud flats. In consequence of these conditions this plain is entirely abandoned, during the winter, to the cold and snows brought by the north winds from the plateaus of Southern Russia and from the Ural Mountains. The proximity of the Black Sea scarcely serves to moderate the severity of this season, and on the banks of the Kuban and Terek the snows of winter are deep and the mercury often falls to $-20°$ Reaumur. During the summer, on the contrary, fair weather prevails, and in this portion of the year the surface of this plain, destitute of forests, is heated to an almost insupportable degree. At times refreshing winds blow from the south and southeast, coming from the mountains or from the Caspian, bringing rain storms and cooling the earth and air. The summer is, however, generally excessively hot, the mercury rising to 30° Reaumur.

The southern portions of the Caucasian Isthmus are completely sheltered from the north winds by the great mountain barrier to which reference has been so frequently made. On the east and west this region is open to the influences of the Caspian and Black Seas, while its climate is somewhat influenced by the wall of mountains which in Northern Persia and Asia Minor bound it on the south. In consequence of these circumstances no considerable degree of cold is felt here. This is specially true of the valleys of the Rion and Kur, where frosts are very rare, and where the mercury never falls below 5° R. Snow almost never covers the country, and when it falls remains but a few hours. The winter is short as well as mild, and generally consists of but a brief rainy season. On the other hand the summer is long and warm, and but for the influence of the neighboring seas the heat would be unbearable.

The mountains of Karthlo-Imeritia exert an influence on the climate of the valleys of the Rion and Kur which deserves to be noticed. From its transverse direction across the great

valley—if such it may be called—drained by the Rion and Kur, it forms a barrier limiting the influence of the Black Sea on the east, and of the prevailing southeast winds which sweep the valley of the Kur. To the protection afforded by this chain the luxuriance and humidity of the valley of the Rion must be ascribed, as but for this obstacle the southeast winds of summer, having parted with their moisture in the lower part of the valley of the Kur, would carry their dessicating influence even to the shores of the Black Sea. The climate of the Little Caucasus, and the valley of the Araxes, on account of their elevation, is a little colder in winter than that of the valleys of the Rion and Kur; but in summer it is perhaps as hot.

The proximity of the Isthmus of the Caucasus to the cradle of the human race, its climate, the richness of its flora and fauna, its mineral resources, and finally the beauty of its scenery, have made it the favorite abode of many nations. Not only do the writings of the ancient historians, Herodotus and Strabo, prove that this country was inhabited at a time very remote from the present, but the traces of human occupation of still earlier date scattered over all portions of it, indicate that anterior to the historic period it was possessed by various races greatly differing in their degree of civilization.

Stone implements are frequently found in the valley of the Araxes, and in the old salt mines of the same region; mines which are regarded as the most ancient known, and as probably the oldest monuments of human industry that now exist.

In the mountains of Karthlo-Imeritia are excavated ("*troglodyte*") houses, and an entire city has been discovered wrought out of the rocky walls which border the narrow valley of the river Ljokhwa, an affluent of the Kur. North of Mt. Ararat, on the summit of a hill, are to be seen the ruins of the city Armawyr, which, according to historical records, was the first capital of Armenia, more than 2,000 years B. C. This city was then situated on the banks of the Araxes, but the river now passes at the distance of seven or eight kilometers. One of my colleagues, M. Guileff, has lately found in the narrow valleys of the Caucasus, not far from the eastern shore of the Black sea, certain graves known in Europe under the name of Celtic tombs. Among the inhabitants of this part of the Caucasus the tradition exists "that these so-called tombs were the habitations of a people who lived here, and that they were built for them by their neighbors the giants." On the peninsula of Taman an embankment or rampart exists, which is attributed to the Cimmerians, who, according to the ancient chronicle, inhabited the shores of the Euxine sea. Scythian remains are found throughout southern Russia, and as far south as the base of the Great Caucasus.

Well marked traces of the presence in this country of a people far advanced in civilization have remained to us from the time of the early Greek colonies. These colonies occupied all the valley of the Rion (the ancient *Phasis*), the peninsulas of Taman and Kertsch, and a portion of the course of the Kuban (the ancient *Hypanis*). That these were rich and flourishing colonies we learn not only from the narrative of the expedition of the Argonauts, but much more conclusively from the relics which they have left behind them, and which clearly indicate wealth and a cultivated taste. These relics are usually found in tombs, and the tombs in sepulchral mounds. They consist of pottery, implements, arms, ornaments in gold or silver, coins, statues, etc.

In the year 1864, I had occasion to assist in the opening of two royal tombs which had been found in one of two tumuli, called the twin tumuli, situated upon the peninsula of Taman. When by boring a mass of masonry had been detected in one of these tumuli, two horizontal galleries were carried through it, crossing each other at right angles, by means of which two separate tombs were discovered. These tombs contained the following relics: In the middle of each was a sarcophagus of cypress-wood, in the form of an oblong box without cover, and standing upon small feet. The external surfaces of these sarcophagi were covered with a carved and gilded ornamentation. In each was a skeleton which crumbled into dust at the slightest touch, also the remains of clothing and sandals, and various golden ornaments. Among these were masks made of thin plates of gold, which had been molded to the features so as to represent the physiognomy of the wearers. Above them were two crowns, which with the masks were indicative of a difference of sex in the persons buried. One of these crowns represented a garland of oak and laurel branches. The second also formed a garland, but composed of ears of wheat, and flowers. The golden ornaments found in the sarcophagus of the female were richest and most numerous; as for example, beneath her mask was a collar formed of several rows of chains interlaced. To these chains were suspended charms representing divinities, animals and other fantastic figures. In the same sarcophagus were found bracelets for the arms and limbs. The combined weight of the golden ornaments it contained was 32 kilograms. In the sarcophagus of the male, besides the mask and crown, the only ornament found was a girdle also of gold. The other objects contained in the tomb clearly indicated the difference of sex which has been alluded to. In one there were placed around the sarcophagus and along the walls urns, vases, perfume bottles, lacrymatories, metallic mirrors, and shells con-

taining pigments. There were also statues in terra-cotta representing the following divinities: Cupid, the Three Graces and Venus, besides a large statue in white marble of the goddess Aphrodite. In the other there were amphoræ, vases, platters of different sizes of black pottery ornamented with red figures, arms eaten up by rust, and small figures in terra-cotta representing the goddess Diana. Both the tombs in which these relics were found were built of dressed stone, the material having been obtained from the quarries of Tertiary limestone on the peninsula of Kertsch. They were not arched above, but the ceilings were composed of courses of stone which overlapped till they met in the center. These tombs were placed a little above the base of the tumulus, which was of a conical form, from 35 to 45 feet in height and 70 feet in diameter, and composed of successive layers of carbonaceous soil, of sand and fragments of broken vases, mixed with ashes and charcoal. Among the fragments of pottery were many which seemed to have formed part of a large and complicated design. This excited a desire to reunite them, and since they were scattered throughout the whole mound, to accomplish this end it became necessary to sift the entire mass. This gigantic labor occupied 150 men for several months, and resulted in complete success. From the broken fragments were reconstructed a large vase and a flat dish bearing different designs, but referring to one episode in the history of Greece. Upon the dish was portrayed the meeting of Paris and Helen, and upon the vase the abduction of the beautiful wife of Menelaus. From their size, the beauty of the conception and the execution of the ornamentation, this vase and dish are unquestionably the finest specimens of the kind yet discovered.

The mounds containing tombs similar to those described are mostly found on the peninsula of Kertsch, in the suburbs of the town of the same name, which was formerly the site of Panticapæum, and on the peninsula of Taman, near the sites of the ancient cities of Corocondama and Phanagoria, of which almost no other traces remain.

After the remains of the Greek colonization come those which belong to the propagation of Christianity. Of this epoch we have traces in nearly all parts of the Caucasus. The Genoese occupied not only the peninsula of the Crimea, but they were scatterod through the valley of the Rion, and over the northern slope of the Great Caucasian chain north of Mt. Elbruz. The pass of Moroukh served them as a route of intercommunication between the colonies north and south of the mountains. Along this route, and in general in all the western portions of the Isthmus are found ruins of churches and monasteries, sometimes with paintings on the walls, sepulchral

monuments, inscriptions, ornaments and utensils. The most remarkable remains of the period of the propagation of Christianity among the Armenians and Georgians are the ruins of the city of Ani, the ancient capital of Armenia, situated on the banks of the river Arpa-tschai.

Nearly all the ancient remains found in the eastern part of the Caucasus belong to the epoch of the domain of the Persians and consist of ruins of mosques and caravanseras, palaces and fortifications, aqueducts, and canals.

The height of the mountains, and the isolated and enclosed character of the valleys of the Isthmus of the Caucasus may be considered as the principal cause of the diversity in the population of the Caucasus and of the tenacity with which national characteristics have been maintained.

The northwest part of the Caucasian chain, as far east as Mt. Elbruz, is inhabited on the north side by the Netokhasch, Schapsaughes, and Abadzek; on the southern slope by the Oubjiskh and Abkhaz.

After the subjugation of this portion of the Caucasus, in 1864, the greater part of these tribes, except the Abkhaz, quitted the country, and it has since been occupied only by the Russains. All the tribes mentioned speak different languages.

Judging from certain customs which are retained by these people, we are led to believe that during the time of the Gennis colonies Christianity spread widely among the inhabitants of this part of the Caucasus, but now they are all Mohammedans, and belong to the sect of the Sunnites.

Between the meridians of Elbruz and Tebaulas-mtha, on the northern slope of the mountains, and along the river Terek and its tributaries are some Russians who form military colonies; the original population consisting of the inhabitants of Kabarda and Tschetschnia. The valleys among the mountains in this part of the chain are inhabited by the Ossethes and the Torschethes.

On in the basin of the Rion are found the Mingrelians and the Imerethians, in the valley of the Kur, the Georgians, and further south in the mountains of Kathlo-Imeritia and the Little Caucasus, the Gourians and Armenians. All these speak different languages, and are Christians, except the inhabitants of Kabarda and Tschetschnia, who are Mohammedans. The southeastern portion of the Isthmus of the Caucasus is occupied, in the valley of the great mountain chain by a population known under the general name of the inhabitants of Daghestan.

This population scarcely represents a single nation, but is rather composed of several tribes who inhabit different valleys, speak different dialects of one language, and who are all Mohammedans of the Sunnite sect. The southern slope of the

eastern part of the Caucasian chain and the Little Caucasus are inhabited by a people descended from the Persians, who speak one language, are Mussulmans of the sect Slhüte. In addition to the indigenous tribes which have been enumerated, there are in the Caucasus many Russians and Germans who form communities scattered through all parts of the country.

The occupation of the Isthmus of the Caucasus for ages by so many nationalities, has made it the scene of incessant strife, by which its productiveness and prosperity, so highly favored by nature, have been greatly impeded.

Except the working of mines and the manufacture of metals, the industry of the country is absorbed in agriculture, stock raising, fishing, rearing of silkworms, horticulture and the manufacture of wine.

At the north, on the plains drained by the Terek and Kuban which are without forest, and have a dark, rich soil, wheat is generally cultivated—rye sparingly, and barley, oats, and buckwheat are grown.

Along the rivers when irrigation is easy, fruit and vegetables are raised; but only in the vicinity of the cities Pjotigorsk and Kyzljar is the vine cultivated and wine made.

As the vast plains bordering on the Terek and Kuban consist largely of prairies, the raising of horned cattle and horses is there extensively followed.

In the Great and Little Caucasus, in consequence of the physical conditions which prevail there, the inhabitants devote themselves to the raising of sheep, and the cultivation of the soil is of secondary importance.

In the southern portions of the Isthmus, and especially in the valleys of the Rion, Kur and Araxis, where nature has been more generous in her gifts, the range of productive industry is much extended. As the climate is mild, and the soil good, it may be said that the productiveness of this region is dependent simply on the amount of water available for irrigation. Beside wheat, barley and buckwheat, rice, cotton and maize are raised here. Horticulture is also largely practised, and the cultivation of the vine assumes great importance, formerly the principal occupation of the Cakhethii who live along the Alazan, one of the tributaries of the Kur.

The cultivation of tinctorial plants such as indigo and saffron is followed on the western shore of the Caspian.

In the localities most favorable to the cultivation of fruits much attention is also given to the rearing of silkworms. The most important centers of this industry are the cities of Schemakha and Naukha.

Upon the shores of the Caspain and Black seas, and the sea of Azoff, especially on the peninsula of Taman, and at the

mouths of the Terek and Kur are important fisheries, which compensate in some degree the inhabitants of these districts for the marshy and unproductive nature of the surface.

Since the Russians have gained control of the Caucasus, the Government has taken great pains to foster all forms of productive industry, but its efforts have been much impeded, and often completely thwarted by the perpetual war which the mountaineers have maintained. In 1864, however, the last of the opposing forces were overcome and with the general restoration of peace it is hoped that the development of the resources of the country will advance with more rapid strides. This may be the more confidently expected since all the power of the government hitherto absorbed by war is now directed toward the organization of society, and the amelioration of the condition of the country. Within the last few years the Russian authorities have been engaged in the emancipation of the serfs, the organization of a system of public instruction, in repairing and constructing canals for irrigation, roads, etc. Among these provisions for the public good, one of the most important is the proposed rail road from the Black Sea to the Caspian. This will give a new impetus to the intellectual and industrial development of the country, hitherto so much retarded by the want of unity and harmony among its heterogeneous population.

Note.—A convenient map for consultation in connection with this article may be found in Haxthausen's Transcaucasia, (2 vols. 8vo. Leipsic, 1856.) A profile of the Caucasus range, with the heights of numerous peaks, may be found in Petermann's *Mittheilungen* for 1859, (tafel xii,) based upon the data of General Chodzko. Some of Abich's papers on the geology of the Caucasus are given in the Berlin Zeitscrift für Erdkunde. One of these, in the volume for 1853, is illustrated by a profile, colored geologically, of the slope of the Caucasus, north from Elbruz toward Beschtau.—Eds.

Art. XXXIII.—*Notes on Mr. Charles Stodder's paper entitled "Nobert's Test-plate and modern Microscopes," published in the American Naturalist*, April, 1868; by W. S. Sullivant.

Mr. Stodder's paper above cited is full of interest to the microscopist. In it is announced the resolution of lines on the Nobert Test-plate* which are as close together as the $\frac{1}{112,595}$ of an English inch, and much exceed in fineness those heretofore seen by other observers.

* The plate used in the trials detailed by Mr. Stodder is one of nineteen bands, the first being ruled to the $\frac{1}{1000}$ of a Paris line or to the $\frac{1}{11260}$ of an English inch, each band increasing by 500 so that the 19th band is ruled to the $\frac{1}{10000}$ of a Paris line or to the $\frac{1}{112595}$ of an English inch.

From Mr. Stodder's brief sketch of what had been previously done in the separation of lines under the microscope, it appears that Ross, De la Rue, and Sullivant and Wormley after extended and exhaustive experiments on the Nobert test plate, failed to resolve lines closer together than about the $\frac{1}{89,000}$ of an inch. It appears also that Nobert himself has never been able, with the highest powers, to see lines on his own test-plates closer together than the $\frac{1}{84,400}$ of an inch.

On the other hand, the late Professor J. W. Bailey claimed to have seen lines the $\frac{1}{100,000}$ of an inch apart; and Messrs. Harrison and Sollitt claim to have measured striæ on the diatom *Amphipleura pellucida* having an interval of the $\frac{1}{120,000}$ to the $\frac{1}{130,000}$ of an inch, and gave it as their opinion that lines as close as the $\frac{1}{175,000}$ of an inch could, with proper means, be resolved. The above is learned from Mr. Stodder's paper.

There is no reason to question the results, such is their close accordance, obtained by the observers first mentioned, but in reference to the claims made by Prof. Bailey and Messrs. Harrison and Sollitt, it may be remarked that Prof. Bailey, though one of the most accomplished physicists of his day, was, owing doubtless to defects in the apparatus used, quote inaccurate in some of his micrometrical measurements;* and with regard to the alleged measurements of the striae of *Amphipleura pellucida* by Messrs. Harrison and Sollitt, it is sufficient to say that it is now the generally received opinion among microscopists familiar with that diatom, that no true striæ have yet been seen on its frustules.†

From the foregoing it is not an unwarrantable inference that anterior to the experiments reported in the paper under notice, no satisfactory evidence is on record that lines closer than about the $\frac{1}{89,000}$ of an inch, either on Nobert's test-plate or any other object, have been resolved. This being the limit reached by previous observers, the skill in manipulation and in the management of the illumination, &c., that brings to view, so distinctly and palpably, lines ruled to the $\frac{1}{112,000}$ of an inch that they have actually been counted and measured and found to

* He assigned to *Pleurosigma Spencerii* a striation of $\frac{1}{120000}$ to $\frac{1}{130000}$ of an inch, the real striation being only about $\frac{1}{50000}$ of an inch. (This Journal, Jan., 1850). The striae of *Grammatophora subtilissima* are given by him as $\frac{1}{120000}$ to $\frac{1}{130000}$ of an inch apart instead of about $\frac{1}{75000}$, the true distance, (this Journ., Jan., 1851.)

† Mr. Sobb, (*Carpenter on the Microscope*, 3d ed., p. 198) claims to resolve *Amphipleura pellucida*. Prof. H. L. Smith, of Gambier, Ohio, whose valuable additions to microscopical apparatus are well known, has given much attention to this diatom; he recently witnessed Mr. Sobb's alleged resolution of its frustule, and considers the lines exhibited as spectral or spurious, and such, he states, is the opinion of Mr. Wenham.

correspond with the registration on the test-plate (p. 100), challenges the admiration of all interested in microscopy, and proves by the inexorable test of experiment, that the resolution of such lines is not incompatible with the physical properties of light, as has been asserted by Fraunhofer and other writers of authority on optics.

The grade of some of the objectives with which these resolutions were made is scarcely less remarkable than the resolutions themselves. Reliance was placed, not so much on those beautiful achievements in optical art, the $\frac{1}{25}$ and the $\frac{1}{50}$ lately sent out by Powell and Lealand, as on objectives of a medium grade, such as a $\frac{1}{6}$ *immersion* and a $\frac{1}{5}$ *dry* by Tolles. Mr. Stodder says "these trials show conclusively that it is not the great power of the objective that is important, for in many of the trials here reported, the lower powers have given the best results, but the skill of the opticians in making the instrument."

The objectives of Mr. Tolles unquestionably rank among the best, but it may be doubted if evidence exists, unless it be these trials reported by Mr. Stodder, of their superiority to those made by Spencer in this country, and by Powell and Lealand and others in England. Hence it is a fair inference that the failure of previous efforts on the highest bands of the Nobert test-plate is attributable to causes other than an incapacity in the objectives used.

Mr. Stodder would have done an acceptable service to those who may hereafter attempt such investigations, had he gone somewhat into detail as to the system of illumination, the auxiliary apparatus, &c., adopted by himself and other gentlemen whose experiments he reports.

Such success in carrying up the resolvability of lines so far beyond the point at which well-directed efforts, sustained by theory, had placed it, will doubtless attract other observers to this field of research, where, among other sources of deception, none will be found more prolific than the spurious or spectral lines always shown by the objective working with oblique light and under a strain; for it may well be supposed that any objective, especially one so low as a $\frac{1}{5}$th would be strained in the resolution of lines ruled to the $\frac{1}{112,000}$ of an inch.

It is well known to all familiar with this subject, that it is impossible to distinguish, by their mere visual appearance, the spurious from the true lines on the highest bands of the Nobert plate. No lines, therefore, should be entitled to full confidence as being the true lines, unless verified by the micrometer, that is, counted, measured, and found to correspond with the registration on the test-plate.*

* It is here assumed that the lines are ruled on the test-plate as indicated by Nobert; no error having yet been found on any of his plates.

Mr. Stodder remarks "it has been said that the resolution of lines to the eye, implies the ability to count them, but this, I think, is a fallacy," and illustrates his remark by the difficulty of counting the pickets on a fence, at a given distance. The difficulty in both cases could perhaps be surmounted to the extent necessary, by enlarging the visual angle under which the lines and pickets appear to the eye, viz: by adding to the amplification of the microscope, and shortening the distance to the fence.

Among the highest bands of the Nobert plate, owing to the want of perfect flatness of field inseparable from the best objectives, a portion only of the width can at one time be brought into exact focus. If that portion, however, is measured and its lines resolved under a suitable amplification, the data are obtained for the solution of the problem in hand, namely, the determination of the distance apart of the lines thus separated.

In another extract from Mr. Stodder's paper he says "in counting lines of such exquisite fineness, either the micrometer or the stage must be moved, and it is next to impossible to construct apparatus that can be moved at once $\frac{1}{100,000}$ of an inch and no more." This remark, coupled with the one above cited in which the supposed ability to count lines resolved to the eye is deemed a fallacy, suggests the inquiry, how was it satisfactorily ascertained that the *true* lines on the 19th band, ($\frac{1}{112,688}$) were seen (page 99) with a $\frac{1}{6}$ objective and under an amplification of 550 diameters?

Besides the low grade of the objective, a noteworthy feature in this performance, is the low amplification employed. Heretofore it has been found no easy task to confirm by count and measurement, lines $\frac{1}{83,000}$ of an inch apart, resolved by high-grade objectives, under an amplification of 6,000 diameters.

Mr. Stodder very correctly remarks that an exact and controllable motion in the micrometer or the stage for the purpose of counting the lines of the highest bands, is next to impossible. But in the mere counting of lines, amplification is the important requisite, not the micrometer, the office of which is simply to measure that portion of the width of a band in which the lines can be counted. Motion in the measuring apparatus —the cobweb-micrometer, for instance—is required for the purpose mainly of setting the spider-lines so as to embrace accurately, and thus measure the portion of the band above mentioned, not for the purpose of measuring off, line by line, one at a time, the Nobert lines, as Mr. Stodder seems to require: this, indeed, as before said, would be next to impossible.

But there are other methods of measurement, and it may be safely asserted that whatever lines the objective can resolve,

amplification with illumination for counting them, and apparatus for measuring the space in which they are counted, are all within the reach of the microscopist.

Mr. Stodder's views on the micrometry of the Nobert lines, are certainly untenable. He invests the subject with insurmountable difficulties, and thus seems to ignore the only certain and reliable means of determining the nature, whether real or spectral, of any lines that may be seen on the high Nobert bands.

His paper, nevertheless, will form an interesting part of the literature of a subject that has long attracted much attention, namely, the limit of the resolvability of lines under the microscope. The experiments recorded by Mr. Stodder go far toward determining this limit. They show that there was an error in fixing it among lines about the $\frac{1}{85,000}$ of an inch apart. They also show that lines as close as the $\frac{1}{112,000}$ of an inch can be separated. These are the finest lines ever yet ruled on any test-plate. How much narrower an interval lines may have and still be resolved, cannot probably be determined with numerical precision, until the ingenuity of Nobert adds finer ruled bands to his test-plates.

In the meantime, microscopists will doubtless find "pleasant divertisement" in resolving with their $\frac{1}{5}$ths and $\frac{1}{8}$ths, the lines of the four or five highest bands of the present test plate; and they would do well in dealing with even these bands to bear in mind that "no other sense approaches in power of self-deception to that of sight; and that, especially when the eye is strained by an eager observer, and the imagination perhaps, plays under the pressure of a theory, it is quite possible, after a little, to see almost anything that is expected."

Note.—Since the above was in type the writer has been kindly presented by Dr. J. J. Woodward of the U. S. Medical department at Washington, with a series of beautiful photographs, recently made by his assistant Dr. E. Curtis, of all the bands of Nobert's 19-band test-plate. The first 15 bands are sharply and clearly resolved into their true lines; the 15th band, however, (which is ruled to the $\frac{1}{90000}$ of an English inch) requiring a hand-glass, magnifying four or five diameters, to show its lines distinctly.

The resolving and photographing such extraordinarily fine lines rank first among any performances of the kind on record, and attest the remarkable skill of Dr. Curtis who has accomplished both so successfully.

The objective used was a $\frac{1}{25}$ inch made by Powell and Lealand; amplification 1,000 to 2,000 diameters. The photographs of the 16th, 17th, 18th, and 19th bands gave, as Dr. Woodward remarks, only false or spectral lines.

ART. XXXIV.—*Remarks on the nineteen-band Test-plate of Nobert;* by J. J. WOODWARD, Asst. Surgeon, and Brevet Lieutenant-Colonel U. S. Army.

THE January number of this Journal for 1861 contains an interesting article on the thirty-band test-plate of Nobert, by W. S. Sullivant and T. G. Wormley.

In this test-plate the lines of the first band are ruled $\frac{1}{1000}$th, those of the thirtieth band the $\frac{1}{8000}$th of a Paris line apart, measuring from the center of one line to that of the next.

Sullivant and Wormley resolved the first twenty-six of these bands, partly resolved the twenty-seventh, but failed to resolve satisfactorily the last three bands.

Nobert has since prepared a test-plate of nineteen bands, in which the lines of the first are the $\frac{1}{1000}$th, those of the second the $\frac{1}{1500}$th, of the third the $\frac{1}{2000}$th, and so on, those of the nineteenth band being the $\frac{1}{10,000}$th of a Paris line apart.

In this new plate the fifteenth band corresponds precisely with the last band of the thirty-band plate, the lines being the $\frac{1}{8000}$th of a Paris line apart.

On one of these new plates Max Schultze * has succeeded in resolving the fourteenth band, Eulenstein of Stutgard has done the same, and Nobert himself has gone no further,† nor, so far as I know, has any other European microscopist.

Under these circumstances, Mr. Charles Stodder of Boston tells us that he and Mr. Greenleaf "saw the nineteenth band satisfactorily" with a Tolles' $\frac{1}{8}$th immersion of 170° angle, magnifying 550 diameters.

They did not count the lines, an operation which Mr. Stodder thinks is quite impossible.‡

For myself, having also seen lines which I thought real in the nineteenth band, with several objectives, and having satisfied myself both by counting and by having photographs taken of what I saw, that these were spectral or spurious, and not real lines, I am quite convinced that those which Messrs. Stodder and Greenleaf have seen are of the same nature, and hope these gentlemen will attempt to count the lines they see in the nine-

* Archiv für mikroskopische Anatomie. Bonn, 1863, p. 305. His words are: "Bei schiefem Licht bin ich mit den besten systemen bis zur 15ten Gruppe gekommen."

† See quotations from a letter of Eulenstein in the paper of Mr. Charles Stodder hereafter quoted.

‡ Nobert's test-plate and modern microscopes, American Naturalist, vol. ii, p. 97, also Microscopical Journal, July, 1868, p. 131. To the latter article Mr. Stodder appends a note in which he claims that Dr. Barnard also has seen the real lines in the nineteenth band. I am quite sure the lines Dr. Barnard saw were also spurious ones, and he himself writes me that his opinions in the matter were not fully matured, and that he intends to make further observations.

teenth band by the very simple method I shall append, or to make a photograph of them, either of which will, I think, speedily bring them to my opinion.

I have lately made a careful study of a nineteen-band plate belonging to Rev. Dr. F. A. P. Barnard, which was made for him by Nobert during 1867.

To convince all microscopists interested of the accuracy of my statements, I have had photographs of the different bands made by Dr. E. Curtis, at the Army Medical Museum.

I am satisfied that I have seen, and that Dr. Curtis has photographed the true lines in the fifteenth band, but no further. Dr. Curtis has also photographed two views of the sixteenth, seventeenth, eighteenth and nineteenth bands, in which lines are clearly seen, which, however, I conclude from their number and character to be spurious, although both he and I supposed them to be real before counting them by the method I shall presently mention. For example, the spurious lines in the photograph of the seventeenth band are 30 in number in one photograph; 40 in another; the real number ot lines according to Nobert's statement being 51.

I send herewith a full set of these photographs to the editors of this Journal.

In these photographs the lines count as follows in the several bands:

First,	7 lines.	Eighth,	25 lines.	Fifteenth,	45 lines.
Second,	10 "	Ninth,	27 "	Sixteenth,	not counted.
Third,	13 "	Tenth,	30 "	Seventeenth,	" "
Fourth,	15 "	Eleventh,	34 "	Eighteenth	" "
Fifth,	17 "	Twelfth,	37 "	Nineteenth,	" "
Sixth,	20 "	Thirteenth,	40 "		
Seventh,	23 "	Fourteenth,	43 "		

The photographs were taken with the $\frac{1}{25}$th of Powell and Lealand, and a distance sufficient to magnify 1000 diameters linear. The slide was illuminated by direct sunlight passed through a solution of sulphate of copper in ammonia, and concentrated by an achromatic condenser, with large diaphragm opening, without any central stop, the pencil made oblique by throwing the condenser to the right or left of its true centering. I tried on the slide many object glasses, including an $\frac{1}{8}$th of Ross, a $\frac{1}{10}$th of Tolles, a No. 11 immersion of Hartnack, an $\frac{1}{8}$th, $\frac{1}{15}$th and immersion $\frac{r}{10}$th by Wales, but got the best results with the $\frac{1}{25}$th. The $\frac{1}{50}$th of Powell and Lealand did not do quite so well as the $\frac{1}{25}$th, apparently on account of the thickness of cover.

Dr. Curtis has also made two photographs of the twelfth, thirteenth and fourteenth bands, in which the resolution being

imperfect, spurious lines are shown. The thirteenth band, for example, shows 25 lines in one photograph, and 16 in the other, the real number being 40.

These two photographs agree closely in character with those of the sixteenth, seventeenth, eighteenth and nineteenth bands.

The foregoing results lead me to believe that the lines in the last four bands are really ruled as claimed by Nobert; and that with greater defining power the true lines could be seen. They also compel me to doubt the accuracy of the statements of those who think they have seen the true lines in any band beyond the fifteenth, and especially if the lines seen have not been counted.

I may here mention that the first of this series of photographs represented the last four bands, and was made by Dr. Curtis some months ago. Both he and I supposed at the time that the lines shown on the sixteenth and seventeenth band in this photograph were real. We accounted for their being too few in number (those of the sixteenth counting 37, those of the seventeenth, 40 lines), by supposing that the whole of each band was not to be seen in any one position of the focal adjustment.

Subsequent experience has, however, convinced us that these also are spurious lines. I learn from Dr. Barnard that this photograph was sent by Eulenstein to Nobert, who accounted for the small number of lines by supposing Dr. Curtis had by mistake copied the twelfth, thirteenth, fourteenth and fifteenth bands! We had, however, made no such error, of which I doubt not Nobert will be convinced on receiving the present series of photographs, copies of which I have sent to him.

In conclusion, I may mention briefly two modes of conveniently counting the lines in the highest bands that can be resolved.

If a cobweb micrometer is used, the micrometer eye-piece should be firmly clamped in a stand screwed to the table, so that the eye-piece is close to the end of the microscope tube, but does not touch it, a piece of black velvet being used to complete the connection. The motion of the micrometer screw now communicates no tremor to the microscope, and all difficulty in counting the lines seen (whether real or spurious) disappears. Still better than this is the following method: The microscope being set up in a dark room as though to take a photograph, and the eye-piece being removed, the image of the band to be counted is received on a sheet of plate glass in the plate holder, and viewed with a focussing glass, on the field lens of which a black point is marked. As the focussing glass is moved on the plate from side to side, the black point is moved from line to line. The lines may thus be counted with as much ease and precision as if they were large enough to be touched by the finger.

ART. XXXV.—*Notes on the Geology of Southwestern Ontario;* by T. STERRY HUNT, F.R.S., of the Geological Survey of Canada.

(Read before the meeting of the American Association for the Advancement of Science, at Chicago, August, 1868.)

THE paleozoic strata of the southwestern portion of the province of Ontario (late Upper Canada), are generally covered by a considerable thickness of clay, which has made their study extremely difficult. During the last few years, however, numerous borings have been made over a wide area in this region, in search of petroleum, and have disclosed many facts of geological interest. By frequently visiting the localities, and carefully preserving the records of these borings, I have been enabled to arrive at some important conclusions as to the thickness and the distribution of the underlying Upper Silurian and Devonian strata, to which I now beg to call the attention of the Association.

The rocks of the New York series, from the Oriskany sandstone to the Coal, which are regarded as the equivalents of the Devonian of the old world, were shown by Prof. James Hall, in 1851, to constitute three natural groups. Of these, the first and lowest, sometimes called the Upper Helderberg, and consisting of the Oriskany, with its overlying Corniferous limestone (embracing the local subdivision known as the Onondaga limestone), constitutes what may be provisionally called the Lower Devonian. The second group has for its base the black pyroschists, known as the Marcellus shale, followed by the Hamilton shale, with the local Tully limestone, and terminated by another band of black pyroschist, the Genesee slate; the whole constituting what may be termed the Middle Devonian. The third group, embracing the Portage and the Chemung shales and sandstones, with the local Catskill sandstone, makes the Upper Devonian.*

The black Genesee slate, according to Mr. Hall, is paleontologically related to the Hamilton slates, and by him included as part of the Hamilton group, as recognized in *The Geology of Canada.* Similar black slates, though thicker, less fissile, and interstratified with greenish arenaceous beds, occur at the base of the Portage formation, marked by the remains of land plants and of fishes which characterize the Upper Devonian. The black slates at this horizon thus constitute, as it were, beds of passage. The thickness of the lower and more fissile black beds, recognized by Mr. Hall as belonging to the Hamilton group, is, according to him, only twenty-four feet at the eastern end of Lake Erie.

* James Hall, in Foster & Whitney's Geology of Lake Superior, ii, 386.

There exists in southwestern Ontario, along the River St. Clair, an area of several hundred square miles underlaid by black shales, in the counties of Lambton and Kent, of which only the lower part belongs to the Hamilton group. These strata are exposed in very few localities, but the lower beds are seen in Warwick, where they were, many years since, examined by Mr. Hall, in company with Mr. Alexander Murray of the Geological Survey of Canada, and were by the former identified with the Genesee slate forming the summit of the Hamilton group. They are in this place, however, overlaid by more arenaceous beds, in which Prof. Hall at the same time detected the fish remains of the Portage formation. The thickness of these black strata, as appears from a boring in the immediate vicinity, is fifty feet, beneath which are met the gray Hamilton shales. A similar section occurs at Cape Ipperwash or Kettle Point in Bosanquet, on Lake Huron, where bands of alternating greenish and black arenaceous shales, holding *Calamites*, are met with. These strata also were recognized by Mr. Hall, who examined them, as belonging to the Portage formation ; and abound in the large spherical calcareous concretions which occur at the same horizon in New York. The entire thickness of the black shales at this point has not been determined, but in numerous borings throughout the region under notice, they are easily distinguished, both by color and hardness, from the soft gray Hamilton shales which underlie them. At Corunna, near Sarnia, a thickness of not less than 213 feet of hard black shales, interstratified toward the top with greenish sandstone, were met with. In the northern part of Enniskillen, near Wyoming, they are about fifty feet in thickness; at Alvinstone, eighty feet ; in Sombra, on the Sydenham river, 100 feet, and in two borings in Camden, 146 and 200 feet. A little to the north of Bothwell, on the Thames, their thickness was found to be seventy-seven feet, while southward, along the shore of Lake Erie, about sixty feet of the hard black slate overlie the soft gray Hamilton shales.

From these, and a great many similar observations, which are detailed at length in the Report of the Geological Survey of Canada, published in 1866, it has been possible to determine with considerable accuracy the distribution of these black strata beneath the thick covering of clay which conceals them through the greater part of the region. It being impossible, under the circumstances, to distinguish between that lower portion of the black strata which belongs to the Hamilton group or Middle Devonian, and the overlying Portage formation, the whole of these strata, down to the summit of the soft gray shales, are included with the Portage. In Michigan, according to Prof. Winchell, the whole thickness of the Portage (Huron) group,

as just defined, including twenty feet of black shale at its base, is only 224 feet, which are represented in Ontario by 200 feet on the Sydenham river, and by 213 feet at Corunna on the St. Clair. Yet Prof. Winchell, for some reason, doubts the existence of the Portage formation in Ontario.

The Hamilton shale, which in some parts of New York attains a thickness of 1,000 feet, but is reduced to 200 feet in the western part of the state, consists in Ontario chiefly of soft gray marls, called soapstone by the well-borers, but includes at its base a few feet of black beds, probably representing the Marcellus shale. It contains, moreover, in some parts, beds of from two to five feet of solid gray limestone, holding silicified fossils, and in one instance impregnated with petroleum, characters which, but for the nature of the organic remains, and the underlying marls, would lead to the conclusion that the Lower Devonian had been reached. The thickness of the Hamilton shale varies in different parts of the region under consideration. From the record of numerous wells in the southeastern portion, it appears that the entire thickness of soft strata between the Corniferous limestone below and the black shale above, varies from 275 to 230 feet, while along the shore of Lake Erie it is not more than 200 feet. Further north, in Bosanquet, beneath the black shale, 350 feet of soft gray shale were traversed in boring, without reaching the hard rock beneath, while in the adjacent township of Warwick, in a similar boring, the underlying limestone was attained 396 feet from the base of the black shales. It thus appears that the Hamilton shale (including the insignificant representative of the Marcellus shale at its base) augments in volume, from 200 feet on Lake Erie to about 400 feet near to Lake Huron. Such a change in an essentially calcareous formation, is in accordance with the thickening of the Corniferous limestone in the same direction.

The Lower Devonian in Ontario is represented by the Corniferous limestone, for the so-called Onondaga limestone has not been recognized, and the Oriskany sandstone, always thin, is in some places entirely wanting. The thickness of the Corniferous in western New York is about ninety feet, and in southeastern Michigan is said to be not more than sixty, although it increases in going northward, and attains 275 feet at Mackinac. In the townships of Woodhouse and Townsend, about seventy miles west from Buffalo, its thickness has been found to be 160 feet, but, for a great portion of the region in Ontario underlaid by this formation, it is so much concealed that it is not easy to determine its thickness. In the numerous borings which have been sunk through this limestone, there is met with nothing distinctive to mark the separation between

it and the limestone beds which form the upper part of the Onondaga salt group or Salina formation of Dana, which consists of dolomites, alternating with beds of a pure limestone, like that of the Corniferous formation. The saliferous and gypsiferous magnesian marls, which form the lower part of the Salina formation are, however, at once recognized by the borers, and lead to important conclusions regarding this formation in Ontario. In Wayne county, New York, the Salina formation has a thickness of from 700 to 1000 feet, which, to the westward, is believed to be reduced to less than 300 feet, where the outcrop of this formation, crossing the Niagara river, enters Ontario.

At Tilsonburg, ninety miles west from Buffalo, borings have shown the existence of the Corniferous limestone directly beneath about forty feet of clay, while two miles to the southwest, it is overlaid by a few feet of soft shales, probably marking the base of the Hamilton. From a depth of 100 feet in the limestone, at Tilsonburg, a flowing well was obtained, yielding an abundance of water, and a considerable quantity of petroleum. This boring was subsequently carried 854 feet in the rock, which at that depth was a dolomite. Nûmerous specimens from the upper 196 feet were pure non-magnesian limestone; but below that depth dolomites, alternating with pure limestones, were met with to the depth of 854 feet, from which salt water was raised, marking, it is said, from 35° to 50° of the salometer. The well was then abandoned. We have here a boring traversing 854 feet of solid strata, from what was, probably, near the summit of the Corniferous, without reaching the marls which form the lower part of the Salina formation.

In a boring at London, where the presence of the base of the Hamilton was marked by about twenty feet of gray shales, including a band of black pyroschist, overlying the Corniferous, 600 feet of hard rock were passed through before reaching soft magnesian marls, which were penetrated to the depth of seventy-five feet. Specimens of the borings from this well, and from another near by, carried 300 feet from the top of the Corniferous, show that pure limestones are interstratified with the dolomites to a depth of 400 feet. At Tilsonburg a pure limestone was met with at 524 feet from the top.

At St. Mary's, 700 feet, and at Oil Springs in Enniskillen, 595 feet of limestone and dolomite were penetrated, without encountering shales, while in another well near the last, soft shaly strata were met with at about 600 feet from the top of the Corniferous limestone, there overlaid by the Hamilton shales. It thus appears that the united thickness of the Corniferous formation and the solid limestones which compose the

upper part of the Salina formation, is about 600 feet in London and Enniskillen, and farther eastward, in Tilsonburg and St. Marys, considerably greater, exceeding by an unknown amount, in these localities, 854 and 700 feet. The Corniferous at its outcrop in Woodhouse, twenty-five miles to the east of Tilsonburg, measures only 160 feet thick, so that there is evidently, in the localities just mentioned, a great increase in the volume of the Salina formation from the 300 feet observed in western New York. At Goderich, on Lake Huron, the thickness of this formation is much greater. Here are found non-fossiliferous strata, having the character of the so-called Water-lime beds, which belong to the summit of the Salina formation, and are immediately overlaid by fossiliferous strata belonging to the Corniferous formation. At this point a boring in search of petroleum penetrated not less than 775 feet of solid white, gray and blue limestones, chiefly magnesian, with occasional thin beds of sandstone. Below this depth the strata consisted chiefly of reddish and bluish shales, with interstratified beds of gypsum, sometimes ten feet in thickness. After the 164 feet of these, rock salt was met with, interstratified with clay, through a distance of forty-one feet, beneath which the boring was carried five feet in a solid white limestone, probably belonging to the underlying Guelph formation. We have thus, for the entire thickness of the Salina formation at Goderich, 980 feet, of which the upper 775 are hard strata, chiefly magnesian limestones, and 205 feet gypsiferous and saliferous shales. Several wells since sunk in this vicinity, one of them twelve miles to the southwestward, have given almost identical results, including the mass of rock salt at the base. These borings now yield, by pumping, a copious supply of brine, nearly saturated and of great purity, so that this newly discovered saliferous deposit has already attracted the attention of salt manufacturers, both in Ontario and New York. A detailed description of the first well, with an analysis of the brine, will be found in the Geological Report for 1866, already referred to.

Brines are said to have been met with at this horizon in Michigan, where the formation will probably be found to have a much greater thickness than that hitherto assigned to it.

It thus appears that the Salina formation, after being reduced to less than 300 feet at the Niagara river, again assumes, to the northwestward, a thickness of nearly 1,000 feet, and becomes once more salt-bearing, as in the State of New York. The increased thickness of the formation in these two regions, connected with accumulations of salt at its base, would seem to point to ancient basins, or geographical depressions in the

surface of the underlying formation, in which were deposited these thicker portions. The existence of these Upper Silurian salt lakes, whose evaporation gave rise to the rock-salt, gypsum and dolomite of the Salina formation, shows a climate of great dryness to have then prevailed in this region. A similar conclusion is to be drawn from the more or less gypsiferous dolomites of the Calciferous and Niagara formations, the magnesian limestones at other horizons, and the gypsum and salt deposits of the Carboniferous period,—leading us to infer a very limited rain-fall over the northeastern portion of this continent, throughout the Paleozoic period.

In this connection a few remarks with regard to the horizon of the petroleum which issues from the Devonian rocks of Ontario, may not be out of place. In opposition to the generally received view, which supposes the oil to originate from a slow destructive distillation of the black pyroschists belonging to the middle and upper divisions of the Devonian, I have maintained that it exists, *ready formed*, in the limestones below.* In addition to the well known fact of its frequent occurrence in the Corniferous limestone, I have cited the observations of Eaton, Hall and myself, as to the existence of both solid and liquid bitumen in the Niagara limestone, and even in the massive beds of the Hamilton. A remarkable example is afforded in the oleiferous beds of the Niagara formation in the vicinity of Chicago,† and still another in similar strata belonging to the Lower Helderberg period, in Gaspé. The deep borings already mentioned in Tilsonburg, St. Mary's and Enniskillen, showed in each case small quantities of petroleum in strata of the Salina formation, and the same was observed at considerable depths in the Goderich well already described.

Apart from the chemical objections to the view which supposes the oil to be derived from the pyroschists above the Corniferous limestone, it is to be remarked, that all the oil wells of Ontario have been sunk along denuded anticlinals, where, with the exception of the thin black band sometimes met with at the base of the Hamilton formation, these so-called bituminous shales are entirely wanting. The Hamilton formation, moreover, is never oleiferous, except in the case of the rare limestone beds already referred to, which are occasionally interstratified. Reservoirs of petroleum are met with, both in the overlying quaternary gravels and in the fissures and cavities of the Hamilton shales, but in some cases the borings are carried entirely through these strata, into the Corniferous limestone, before getting oil. Among other instances cited in my Geolo-

* Canadian Naturalist, June, 1861, and this Journal, March, 1863.

† It is proposed to give, in a subsequent communication, the results of an examination of this remarkable limestone.

gical Report for 1866, may be mentioned a well at Oil Springs, in Enniskillen, which was sunk to a depth of 456 feet from the surface, and seventy feet in the solid limestone beneath the Hamilton shales, before meeting oil, while in adjacent wells supplies of petroleum are generally met with at varying depths in the shales. In a well at Bothwell, oil was first met with at 420 feet from the surface, and 120 feet in the Corniferous limestone, while a boring at Thamesville was carried 332 feet, of which the last thirty-two feet were in the Corniferous limestone. This well yielded no oil, until, at a depth of sixteen feet in this rock, a fissure was encountered, from which, at the time of my visit, thirty barrels of petroleum had been extracted. At Chatham, in like manner, after sinking through 294 feet of shales, oil was met with at a depth of fifty-eight feet in the underlying Corniferous limestone.

We also find oil-producing wells sunk in districts where the Hamilton shale is entirely wanting, as in Maidstone, on the shore of Lake St. Clair, where, beneath 109 feet of clay, a boring was carried through 209 feet of limestone, of which the greater part consisted of the Water-lime beds of the Salina formation overlaid by a portion of the Corniferous. At a distance of six feet in the rock a fissure was struck, yielding several barrels of petroleum. Again at Tilsonburg, where the Corniferous limestone is covered only by quaternary clays, natural oil springs are frequent, and, by boring, fissures yielding petroleum were found at various depths in the limestone, down to 100 feet, at which point a flowing well was obtained, yielding an abundance of water, with some forty gallons of oil daily. The supplies of oil from wells in the Corniferous limestone are less abundant than those in the overlying shales, and even in the quaternary gravels, for the obvious reason that both of these offer conditions favorable to the retention and accumulation of the petroleum escaping from the limestones beneath.

The presence of petroleum in the Lower Silurian limestones, and their probable importance as sources of petroleum, was first pointed out by me in 1861. The conditions under which oil occurs in these limestones in Ontario, are worthy of notice, inasmuch as they present grave difficulties to those who maintain that petroleum has been generated by an unexplained process of distillation going on in some underlying hydrocarbonaceous rock. Numerous borings in search of oil on Manitoulin Island, have been carried down through the Utica and Loraine shales, but petroleum has been found only in fissures at considerable depths in the underlying limestones of the Trenton group. The supplies from this region have not hitherto been abundant, yet from one of the wells just mentioned, 120 bar-

rels of petroleum were obtained. The limestone here rests on the white unfossiliferous Chazy sandstone, beneath which are found only ancient crystalline rocks, so that it is difficult to avoid the conclusion that this limestone of the Trenton group is, like those of Upper Silurian and Devonian age already noticed, a true oil-bearing rock.

In concluding these observations on the geology of Ontario, it may be remarked that throughout the southwestern counties the distribution of the middle and upper Devonian rocks has been determined almost wholly from the results of borings undertaken in search of petroleum. From these it appears that the wide spread of these rocks in this region is connected, first, with a tranverse north and south synclinal depression, which traverses the peninsula, and has been noticed in the *Geology of Canada*, p. 363, and secondly, with several small undulations, running northeast and southwest, on the northwest side of the anticlinal of the Thames; which is a prolongation of that passing by Cincinnati, and may be regarded as part of the main anticlinal of the great axis of elevation which divides the coal field of Pennsylvania from that of Michigan.

The Devonian rocks are found in the region under consideration, at depths not only far beneath the water-level of the adjacent lakes of Erie and St. Clair, but actually below the horizon of the bottom of those shallow lakes. Thus at Vienna, in Bayham, at a point said to be about forty feet above the level of Lake Erie, the underlying rock was met with beneath 240 feet of clay, while at Port Stanley, twenty feet above the lake, the Hamilton shale was struck beneath 172 feet of clay, and at the Rondeau, just above the level of Lake Erie, the clay was 104 feet thick. A similar condition of things exists on the south side of the lake, at Cleveland, where no rock is encountered at a depth of 100 feet below the water-level. Again in Sombra, on the banks of the Sydenham river, which is a very little above the level of Lake St. Clair, a well ten feet above the river passed through 100 feet of clay before meeting the black shales of the Portage group, while in Maidstone, on the shore of Lake St. Clair, and a very few feet above its level, 109 feet of clay were found overlying the Corniferous limestone. The greatest depth of Lake St. Clair is scarcely thirty feet, and that of the southwestern half of Lake Erie does not exceed sixty or seventy feet, so that it would seem that these present lake basins have been excavated from the quaternary clays which, in this region, fill a great ancient basin, hollowed out of the paleozoic rocks, and including in its area the southwestern part of the peninsula of Ontario.

ART. XXXVI.—*On the action of Sunlight on Bisulphid of Carbon;* by O. LOEW, Chemical assistant in the College of the City of New York.

PURE bisulphid of carbon, when exposed to the sunlight for a considerable time becomes somewhat yellow. To study the changes thus produced, a large quantity of the bisulphid was enclosed in sealed tubes and exposed to the action of the sun. Decomposition took place gradually, and a brown insoluble substance was formed, which adhered so closely to the inner surface of the tubes that it could not be detached by vigorous shaking. This substance prevented the farther action of the sun's rays, and consequently the decomposition ceased.

If water be present in the tubes, this adherence is prevented and a larger quantity of the brown substance is obtained. After an exposure of two or three months the tubes were opened. The water was slightly acid in its reaction, and, after being neutralized and concentrated, it showed a distinct reducing power upon salts of silver and mercury. Evidently therefore a trace of formic acid was produced, according to the following equation:

$$CS_2+2H_2O=\overset{\text{Formic Acid.}}{CH_2O_2}+H_2S+S.^*$$

On filtration, the newly formed brown compound remained on the filter, while the filtrate contained free sulphur dissolved in the bisulphid of carbon. On examination, this compound corresponded in every particular to the *sesquisulphid of carbon*, the substance discovered by me two years ago. It was insoluble in water, alcohol, ether, chloroform, bisulphid of carbon, and oils, but soluble with decomposition in a boiling solution of caustic potassa. On heating it in a glass tube, it was directly separated into its components; the sulphur volatilized and the carbon remained.

If sulpho-carbonate of potassa, in concentrated solution, be exposed to the sunlight, the decomposition is so slight as hardly to be noticed; when the solution is treated with sodium amalgam, however, a reduction to lower sulphids takes place.

In view of the fact that direct sunlight reduces free bisulphid of carbon, it might be supposed that the corresponding body, carbonic acid, would, in presence of water, be reduced in a similar manner; all my experiments in this direction, however, have thus far been unsuccessful. Nevertheless, since this reduction takes place very readily in the tissues of plants under the influence of sunlight, I am not without hope that this process will yet be imitated in the laboratory.

New York, Sept. 20, 1868.

* C=12; O=16; S=32, etc.

ART. XXXVII.—*Observations on the Metamorphosis of Siredon into Amblystoma;* by O. C. MARSH, Professor of Palæontology in Yale College. With a plate.

WHILE on a geological excursion to the Rocky Mountains, during the past summer vacation, the writer obtained at Lake Como, in Wyoming Territory, a number of specimens of *Siredon lichenoides* Baird, one of the most interesting forms of the *Urodela*, or tailed Batrachians, and one hitherto but little studied. This lake is a small shallow sheet of water, distinctly brackish, or "alkaline," apparently from the salts of soda. It lies near the Union Pacific Railroad, about 640 miles west of Omaha, and at an elevation of about 7,000 feet above the sea. The surrounding region is an arid desert, with little or no vegetation except cactus and wild sage.

The Siredons obtained at this locality, where the species, known as the "Fish with legs," is quite abundant, showed at first no differences except those of age and sex. They were from five to ten inches in length, of a uniform dark olive, or pistachio-green color above, and a light olive below. The vascular fimbriæ of the external branchial appendages, or gills, were black. In form and general appearance all the specimens corresponded essentially with the one represented in figures 1 and 1*a*, in the accompanying plate. They were brought to New Haven alive, apparently without suffering much inconvenience, either from a transfer to fresh water, or from the long journey. They all fed readily upon worms and insects, and occasionally came to the surface and inhaled air. More rarely an exhalation occurred, usually under water. On being removed from their native element they soon showed the same signs of distress as fishes under similar circumstances, although in a much less degree.

The account recently published by Prof. Duméril of the remarkable metamorphosis of the second generation of Axolotls (*Siredon Mexicanus*) from the table lands of Mexico, while kept in the Muséum d'Histoire naturelle, in Paris,* made it a point of no little interest to determine whether this species also would undergo a similar change when placed under different physical conditions, and hence the specimens were watched with considerable care.

The first indications of any change were observed in one of the smaller specimens, about six inches in length; and the metamorphosis had apparently commenced during the journey from Lake Como to New Haven, which lasted about a week. Small round spots of dark brown were first noticed on the sides

* Comptes Rendus, tome lxi, p. 775, 1865, and tome lxv, p. 242, 1867.

of the tail, and the color of the entire animal gradually assumed a darker hue. The broad thin membrane along the back, and above and below the tail, gradually began to diminish by absorption, the external branchial appendages soon became similarly affected, especially at the ends, and the animal came more frequently to the surface of the water for air. As the change went on, the dark spots increased in number and size, and gradually extended over the whole upper part of the body. The membrane on the back and tail entirely disappeared, leaving in its place in the dorsal region a sharp groove. The branchiæ also continued to diminish, and at the same time the internal branchial arches began to be absorbed, and shortly after the openings on the neck closed up. In the meantime the head became more rounded above, and more oval in outline, the muzzle narrower and more pointed, and the eyes more convex and prominent. The body also decreased in bulk, and the costal grooves became more distinct. The thin external skin was shed, and the secretion of mucus from the surface sensibly diminished. During these changes the animal showed an increasing desire to leave the water, often remaining for some time with its external nostrils above the surface, and occasionally making violent struggles to escape. Aided by a heavy rain at night it at last succeeded, and thus put an end to further observations, just at a time when it had lost the generic characters of *Siredon*, and become a true *Amblystoma*, two forms of Batrachians usually regarded hitherto as belonging to distinct families.

Fortunately, a few days later, several other specimens of various sizes began, nearly at the same time, to show unmistakable indications of a similar transformation, and this afforded an opportunity of noting the successive phases of the change more fully, as well as of observing the physical conditions which seemed to promote or retard it. Two of the specimens were placed in a glass jar, and left in a strong light, and five others were kept in a cooler place in the shade, the temperature of the two, however, differing but a few degrees. At the end of three weeks those in the glass vessel had apparently completed their metamorphosis, while of the others less favorably situated three only were partially altered, and at the present time, or nearly three weeks later, they still retain tubercular remnants of the external branchiæ, although in most other respects the change appears to be complete. The two remaining specimens, however, which had throughout been kept with the three last, showed no distinct signs of changing, although the probability of their doing so, and the importance of retaining some tangible evidence of the original condition, led to the transfer of one

of them to a jar of alcohol after the first week, a precaution, as the result showed, quite unnecessary in the case of the other, which at the time of writing (Oct. 5th) still remains a typical Siredon, with no alteration more important than a single appearance in a new epidermis.

The changes observed in the five specimens that underwent the complete metamorphosis were essentially the same as those noticed in the one already described, although in no two individuals were the successive phases quite contemporaneous or identical. The most marked differences observed were in the color, both during the transformation, and after its completion. This was very noticeable even in specimens exposed to the same physical conditions. In the siredon state all were apparently precisely alike in this respect, and the first indications of change detected in each case was the appearance of the small dark spots along the sides of the tail. From this stage onward the variation in color in different individuals became very marked. Of the two specimens exposed to a strong light in the glass vessel, one rapidly became covered with dark spots, especially on the back and sides, until finally these predominated, and the grayish yellow of the ground color only remained in vertical and irregular patches (figure 3), the rest being a greenish black. The companion of this specimen, however, although apparently equally advanced in development, retained for some time nearly the original siredon color, the only difference being some irregular blotches on the sides, to which the specific name *lichenoides* would very appropriately apply. These were the extremes of coloring after the change, the other specimens showing various intermediate grades, one individual, represented in fig. 2, having dark brown spots on a light olive ground. All the altered specimens, it should be stated, apparently belonged to the species *Amblystoma mavortium* Baird, as recently defined by Prof. Cope in his able Review of the Amblystomidæ;* and it is an interesting fact that among the six specimens that have already changed, the present coloration appears to indicate two, if not three, of the forms which he there regards as varieties, although previously considered as species by other eminent authorities. The two types best represented are shown in figures 2 and 3. In this connection should perhaps be mentioned a remarkable change of color which took place in one of the Siredons before any indications of metamorphosis had been detected. The animal had been in the dark for several days, and was then placed in a white porcelain vessel, and kept for several hours in a strong light, while an attempt was made to

* Proceedings Philadelphia Acad., xix, p. 166, 1867.

photograph it. During this time it changed from the dark siredon color to a very light yellow. The next morning the original color had returned, but a second exposure produced the same change, which was as speedily reversed on returning the animal to the dark, although it evidently suffered considerably from the treatment. Even a moderately strong light appeared to be distasteful to all the Siredons, and they usually sought the darkest part of the space in which they were confined.

The second distinct phase in the metamorphosis, which, however, commenced in every instance before the change in color had made much progress, was the absorption of the dorsal and caudal membranes. This began on the lower margin of the tail, and soon after could be detected in the dorsal region, and then farther back, the last portion remaining being usually on the upper part of the tail. The absorption extended below the dorsal surface, leaving at first a groove along the back, marking the position of the membrane; and as this disappeared the light colored specimens retained in its place a narrow black line, which extended also to the end of the tail.

The absorption of the external branchiæ was the next marked feature in the transformation, although this also commenced before the changes just mentioned were completed. The atrophy apparently began at the extremities of the branchial processes, and when these had diminished to about half their natural size, and the fimbriæ had disappeared, the ends rolled up underneath, leaving the remnants as rounded tubercles, which, in some of the specimens remained long after all the other stages of the transformation were passed (figure 2). The absorption, moreover, did not always proceed with equal rapidity on the two sides of the same animal, but in one instance stumps of the processes were retained on one side some time after those on the other had disappeared. During the diminution of the external branchiæ, the internal vascular arches which support the external processes also became absorbed. As these disappeared, the orifices on either side of the neck, and the open fold of the throat became closed by the adhesion of the opercular flap to the surface below, leaving, especially in the gular region, a deep cicatrix. These changes, which were in each case apparently completed before the remnants of the external branchiæ had disappeared, were evidently the main cause of a marked alteration in the shape of the head, which occurred about the same time. In the siredon state the head is broadest at the base, and comparatively flat above (figures 1 and 1 *a*), but after the loss of the branchial arches, its greatest breadth was a little back of the eyes, while it was much more rounded

above, and in outline more oval. The neck also diminished in size, and the snout became more pointed. The change in the eyes, already alluded to, likewise altered the appearance of the head materially. The flat, fish-like eye of the Siredon projected very slightly above the surface of the head, but during the transformation this organ became more convex, and also much more prominent (figures 2 and 3). This change in the eye was apparently indicated also in the habits of the animal. The Siredons seldom missed their aim in catching a worm or insect, but when under water after the metamorphosis they often made several ineffectual attempts to seize objects quite near them.

One of the most interesting features of the transformation occurred in connection with the mouth of the animal. The opening, or gape, increased considerably in size, one half at least; the internal and external nostrils became perceptibly more distended, and the tongue, which was at first small, enlarged so as nearly to cover the entire floor of the mouth. An important change also took place in the teeth. In the Siredon the palatine series on the vomerine and pterygoid bones formed an arch nearly concentric with the maxillary row, and extending forward between and beyond the inner nostrils. The arch is interrupted in front, and to some extent on the sides, as represented in figure 1*b*, which shows the position of these teeth, but not their exact number or size. After the metamorphosis, the palatine series project farther from the roof of the mouth, and become much more tranverse, forming an obtuse angle instead of an arch, with the interspaces nearly or quite united (figure 3*a*). The maxillary series also form a somewhat narrower arch, corresponding to the more pointed snout of the Amblystoma, and the splenial teeth of the lower jaw have gradually disappeared. These changes in the dental series, it should be stated, were not in all cases perfectly uniform in different individuals that had apparently passed through the same external phases of transformation, although the tendency was all in the same direction; and hence it is not unlikely that the palatine teeth of some, at least, of the specimens examined, may eventually become still more transverse than those represented in the figure.

Among the other more important changes which occurred during the metamorphosis may be mentioned the decrease in the size of the entire body, which was very marked, a perceptible increase in the distinctness of the costal grooves corresponding to the vertebræ, and the gradual ossification of the carpus and tarsus. The feet also became less palmate, and the toes less depressed. During the transformation, moreover, and espe-

cially after its completion, all the specimens shed the thin, transparent epidermis, some of them very frequently; one, indeed (figure 3), which had been kept in a strong light, lost this covering three times in the ten days immediately following the metamorphosis.

The change in the habits of the Siredon in passing into the Amblystoma state, was scarcely less marked than the alteration in its physical characters. As soon as the absorption of the external branchiæ commenced, the animal came more frequently to the top of the water and took in a mouthful of air; and not long afterwards would occasionally float for some time at an angle of about 45°, with the external nostrils above the surface. Frequent efforts to leave the water soon followed, and an opportunity of so doing was in most instances speedily improved, and the change then seemed to progress more rapidly. One or two specimens, however, showed for some time, especially in cool weather, much less inclination to desert their native element, apparently suffering little or no inconvenience from remaining under water, if allowed to come to the surface about once in five minutes. The pugnacious propensities of the Siredons, which at first led to occasional assaults on one another, appeared to diminish as the change progressed, and the more sluggish nature of Salamanders at last predominated; although the altered forms at times showed no little celerity of movement, and when irritated, especially when held by the tail, would often turn and snap at the hand with a rapidity that would have done no discredit to a reptile of much higher organization.

The effect on the metamorphosis of a variation in light and temperature has already been alluded to. During a succession of very warm days, about the first of September, the change progressed with great rapidity, but it apparently ceased, or made very slight progress, in the cool week that followed. While, moreover, the two specimens most favored in regard to light and warmth passed apparently through the entire transformation in about twenty days, those which commenced at the same time, but were less favorably situated, required at least twice that time for its completion. The only living specimen still remaining unchanged (figures 1 and 1*a*) has twice shown slight indications of an approaching metamorphosis, but with the exception of some spots, these have apparently soon disappeared after a transfer to a dark and cooler place.

Inasmuch as this species of *Amblystoma* appears to have never before been studied from living specimens, and especially as its larva (hitherto known only as *Siredon lichenoides* Baird)

has but very rarely been met with, it may be well to mention some of the more important characters noticed in the individuals just described, in addition to those given in the original description of the two forms, when the connection between them was unknown. It should, perhaps, be first stated that, after the observations here recorded, it becomes at once evident on examining Prof. Baird's excellent description and figures of *Siredon lichenoides*,* that the specimen on which the species was founded cannot be regarded as a typical larva, as it had already made considerable progress in its metamorphosis. This appears to be distinctly shown in the color markings, which suggested the specific name, and likewise in the dorsal and caudal membranes, which have apparently been materially diminished by absorption. The evidence of partial transformation seems to be equally strong, also, in the case of the specimen from Nebraska, subsequently figured and described by Prof. Baird under the name *Siredon melanosticta*, which is possibly only a variety of *S. lichenoides*.† The coloration, the irregular outline of the membranes, the rounded extremities of the branchial processes, as well as the arrangement of the palatine teeth, all indicate that the animal figured had already entered upon the preliminary stages of metamorphosis. In each of these cases, however, it is not improbable that the alteration may have been temporarily, or possibly even permanently, suspended, before the animal was captured. Aside from the features which may be the result of partial transformation, the Siredons from Lake Como do not differ essentially from Prof. Baird's original figures of *S. lichenoides*, except in having a somewhat broader head, and in not having the dorsal membrane extend to the occiput, differences which may be due merely to locality, as the type specimen came from a point about 400 miles farther south.

The Siredons obtained at Lake Como, as already stated, were from five to ten inches in length. The color of the body is a very dark olive above, and a light olive below, while the fimbriæ of the external branchiæ are nearly or quite black. On either side of the body are twelve costal grooves, not including the inguinal. The skin is smooth and transparent, and shows beneath it the ends of innumerable glands, thickly crowded together. In specimens preserved in alcohol these glands project, making the surface appear granular. The dorsal membrane commences a short distance from the base of the head, and both this and the lower membrane extend

* Stanbury's Exped. to the Great Salt Lake, p. 336, 1855.

† Pacific Railroad Report, vol. x, plate xliv, figure 1, and vol. xii, part 2, p. 306.

a little beyond the end of the tail, thus making the extremity slightly emarginate. The carpus and tarsus are unossified. The digits are broad at the base, depressed and triangular. The fourth toe is longest, and has four phalanges.

The head is broad and flat, and the muzzle rounded. The external nostrils are smaller, and slightly nearer together than the interior nasal openings. The eyes are round, and the iris of a yellowish metallic luster. A series of mucus-pores begins near the inner margin of each of the external nostrils, and diverging slightly extends over the nasal and frontal regions. Opposite the eyes they each connect with an orbital series, as shown in figures 1 and 1*a*. Another series, less distinct, exists on either side of the throat, just below the ramus of the lower jaw. Of the four branchial arches, only the three anterior support external processes, and the latter are thickly studded on their lower surfaces with lamellar fimbriæ. The maxillary and palatine series of teeth have already been mentioned, and their position in this species of Siredon is represented in figure 1*b*. The former consists of a single row of slender pointed teeth on the premaxillary and maxillary bones. The palatine arch of teeth, situated on the vomerine and pterygoid bones, forms a narrow and more complex series, only the position of which is given in the plate. The teeth of the lower jaw consist of a single row on the premandibular bones, and an adjoining and somewhat more complex series on the splenial bone. The two close in between the maxillary and palatine arches when the mouth is shut.

The following measurements are taken from two of the Lake Como Siredons. No. 1 is the individual figured in the accompanying plate, and No. 2 a specimen preserved a short time in alcohol.

MEASUREMENTS.

	No. 1. Inches.	No. 1. Lines.	No. 2. Inches.	No. 2. Lines.
Length from snout to end of tail,	7	9	8	
" " " " gular fold,	1	1	1	1
" " " " armpit,	1	8	1	9
" " " " groin,	3	9	3	10
" " " " behind anus,	4	4	4	5
Width of head where greatest,	1	3	1	4
" " tongue,	--	--		8
Distance between eyes anteriorly,		7		7
" " outer nostrils,		5		5
" " inner "	--	--		6
Length of anterior branchial process,		8		7
" " posterior " "	1	2	1	2
Distance between outstretched toes,	3	9	3	8
Height of tail including membranes where greatest,	1	3	1	3

Before the transformation was entirely completed, the generic characters of *Amblystoma* had become unmistakable ; although

in each instance—as not unfrequently occurs in nature—some of the characters which distinguish the species had already preceded them. Among the altered forms, developed from larvæ apparently identical, the two types of coloration, shown respectively in figures 3 and 2, have each at present two representatives, and are sufficiently distinct to merit a more particular description. The former has a ground color of greenish black, on which are bands or patches of grayish yellow, more or less confluent, especially along the back. The abdomen is dusky olive, with a darker medial band. The latter type (figure 2) clearly corresponds to *Amblystoma maculatum* Hallowell, which is regarded by Prof. Cope as a variety of *A. mavortium* Baird. In this form the ground color is light olive, on which are scattered numerous small brown spots. In each specimen, a few of these first appeared on the tail, and next four of larger size on either shoulder, and subsequently others on the sides. The specimens of this type are also larger, and more sluggish in habit than the other variety, and until very recently would have been considered distinct species by all herpetologists. In most other respects, however, there is little or no difference between the two forms, and both of them, with one other specimen representing an intermediate grade, must apparently all be regarded as belonging to Prof. Baird's species *Amblystoma mavortium.**

At the time the Siredons here described were obtained at Lake Como, several others also were secured by Prof. Eustis of Harvard College. All were brought to New York together, and there separated, part being taken to Cambridge, where they have since been carefully observed by Professors Wyman and Eustis, and the rest brought to New Haven by the writer. The former specimens, however, strange to say, have shown but very little inclination to change, none having commenced so doing until after several of the latter had fully developed into Amblystomata, and only the smallest, about five inches in length, having just completed its metamorphosis. This individual, as Prof. Wyman informs the writer, seems still to prefer remaining in the water—doubtless owing in part to the cool autumnal weather—although it is there exposed to attacks from the large Siredons, one of which has twice attempted to swallow it—an interesting fact illustrating the habits of the species. Two of the Siredons kept by Prof. Eustis escaped during a rain storm, and six days afterward one of them was found still alive, although shrivelled up, and the branchiæ partially gone. On being placed in water, it refused food, and soon died. That Siredons of apparently this species are occasionally found in wet grass near the

* Journal Acad. Nat. Sci. Philadelphia, 1849, p. 292.

water, especially after thunder showers, had already been noticed by Dr. Suckley,* but that they can remain out of water for several days is a new and important fact, showing that even before metamorphosis the lungs are sufficiently developed to sustain life without the aid of branchial or dermal respiration.

Whether this species of Siredon ever changes at Lake Como, and in other similar regions, is an interesting question, and one that cannot, perhaps, at present, be answered with certainty. That it does so occasionally, however, under favorable circumstances, especially when young, several facts known to the writer would seem to indicate, although Dr. Suckley, one of the few observers who have hitherto examined it in its habitat, regards it as probably permanently aquatic.† In the elevated region where Lake Como is situated, although the weather in summer is quite warm, the nights are always cool, and the changes of temperature often sudden and very great; hence the metamorphosis, if it began, would probably proceed slowly, and be liable to suspensions during its various stages. That the species, however, breeds in the siredon state, like the Mexican Axolotl, there can be little doubt, although direct evidence on this point appears to be wanting. The observations of Duméril, already alluded to, and other similar facts, render it probable that after reproduction the power of complete development would be lost; although alterations in color and other minor changes might still occur.

The near approximation in many Batrachians of the periods of reproduction and metamorphosis, and the effects, especially upon the later, of even slight differences of physical conditions, as shown in the preceding instances, are known to produce when combined remarkable variations in the same species, as well as other results until recently quite unexpected. The bearing of these and similar facts on a theory of development, although an inviting topic to enter upon, cannot for various reasons be discussed in this connection, but it is evident that in this direction lies a rich field for further investigation. The observations here recorded, however, when taken in connection with those of Duméril on the Mexican Axolotl, render it extremely probable—as Cuvier long ago seems to have suspected‡—that all Siredons are merely larval Salamanders, and also suggests a doubt whether some, at least, of the other so-called Perennibranchiates (by no means a natural division of the *Batrachia*) may not prove eventually to be the undeveloped young of well known species.

In addition to the acknowledgments already made, the writer

* Pacific R. R. Report, vol. xii, part 2, p. 306. † Loc. cit.

‡ Recherches anat. sur les Reptiles, etc., Paris, 1807, p. 35.

desires, in concluding, to express his thanks to Gen. W. Snyder, Superintendent of the Union Pacific Railroad, for his kind assistance in securing the Siredons at Lake Como in August last; to his friend Prof. E. D. Cope, of Philadelphia, for various suggestions in regard to the subject here treated of, and likewise to his friend, Prof. George F. Barker, of Yale College, for careful observations on the specimens while he was temporarily absent from New Haven.

Yale College, Oct. 10th, 1868.

EXPLANATION OF THE PLATE.

Figure 1. Undeveloped larva of *Amblystoma mavortium* Baird, hitherto known as *Siredon lichenoides* Baird. Animal represented as in motion, with external branchiæ thrown back (½ natural size).

Figure 1*a*. Dorsal view of same specimen when at rest, with branchiæ fully expanded (½ natural size).

Figure 1*b*. View from below, showing arrangement of maxillary and palatine series of larval teeth and inner nostrils of same species (natural size).

Figure 2. *Amblystoma mavortium* Baird, (variety *A. maculatum* Hall), partially developed from *Siredon lichenoides* Baird, with remnants of external branchiæ, and internal arches, still retained (½ natural size).

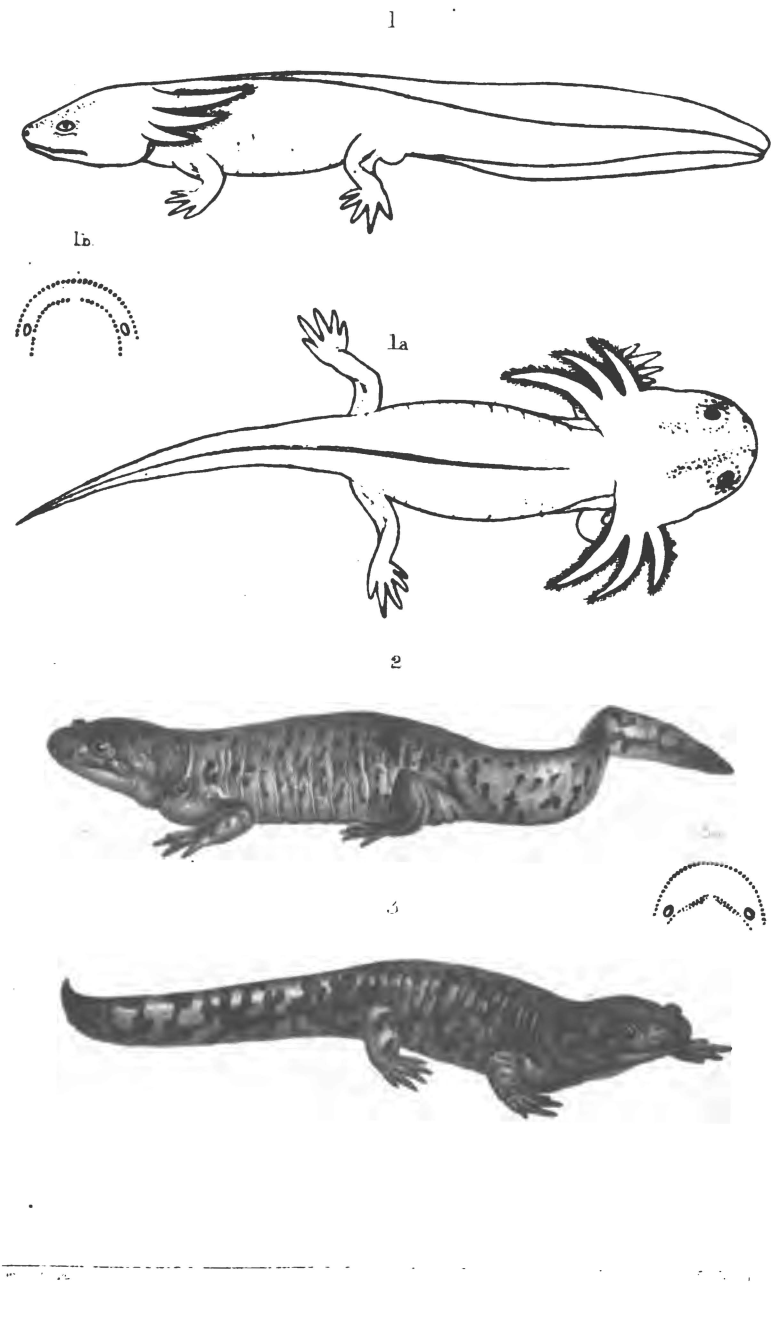
Figure 3. *Amblystoma mavortium* Baird, developed from *Siredon lichenoides* Baird: metamorphosis apparently completed (½ natural size).

Figure 3*a*. Maxillary and palatine series of teeth of *Amblystoma mavortium* Baird, after metamorphosis (natural size).

ART. XXXVIII.—*Notice of a new and diminutive species of fossil Horse (Equus parvulus), from the Tertiary of Nebraska*; by Prof. O. C. MARSH, of Yale College.

IN a small collection of fossil vertebrate remains, obtained by the writer during the past summer in the Tertiary deposits of Nebraska, there are several specimens of no little interest, as they indicate a new species of fossil horse, very much smaller than any hitherto known. These remains were collected at Antelope station on the Union Pacific Railroad, about 450 miles west of Omaha, where a few weeks before, during the excavation of a well, they had been thrown out from a depth of sixty-eight feet. This locality has since attained considerable notoriety, from the fact that the remains then found were pronounced to be human by those who first examined them, and various accounts of the discovery have been published in the newspapers. This, in fact, induced the writer, when in the vicinity, to examine the locality and its fossils, an account of which he has already given elsewhere.*

* National Academy of Sciences. Northampton Meeting, Aug. 1868.


1
1b.
1a
2
3

The equine remains now to be noticed consist mainly of bones of the limbs, and among them is a hoof-phalanx, a coronary or second phalanx, parts of the first phalanx and metacarpals, as well as some of the smaller carpal and tarsal bones, and fragments, apparently from other parts of the skeleton. All are in an excellent state of preservation, and part of them are so characteristic that they clearly indicate the near affinities of the animal to which they belonged.

The ungual or hoof-phalanx differs in form from that of the recent horse only in being somewhat more depressed, and in having the sides of the upper surface slightly less convex transversely, and the beak of the articular face a little less pointed. Its length measured along the axis is very nearly one inch, the shorter diameter of the articular face is five lines, and the longer, or transverse, ten lines. The coronary, or middle phalanx, is proportionally more elongated than in the living species, and its proximal end rather more triangular. Its length along the axis in front is nine lines, the width of the articular face of the proximal end ten lines, and that of the distal end nine lines. The dimensions of all, or nearly all, of the remaining bones render it very probable that they belonged to the same individual, or at least to one of similar size, and specifically identical. They indicate an equine animal scarcely more than two feet, or possibly two and one-half feet in height, although full grown, as the ossification of the various bones clearly proves. Additional parts of the skeleton, especially the teeth, would perhaps show generic characters different from those of the living horse, but in the absence of these, as the remains are evidently distinct from any hitherto described, the species may be named *Equus parvulus*. This makes seventeen species of fossil horses now known to have lived in North America, although until quite recently it was very generally believed that there was none indigenous to the continent.

The bones above described occur in a stratum of gray arenaceous clay, lying nearly horizontally, and apparently of later Tertiary age. The large number of vertebrate remains found together in the space of a few feet indicates a remarkable locality, which unfortunately cannot again be reached except by deep excavation; and hence it is greatly to be regretted that so many of the specimens should have been lost to science by being carried away as human relics. Among those secured by the writer, in addition to the equine fossils, were the remains of several species of ruminants, a phalanx of a carnivorous animal about the size of a lynx, and fragments of a land turtle resembling somewhat the *Testudo neobrarensis* Leidy, all of which will be more fully described in this Journal at an early day.

Yale College, Oct. 5th, 1868.

ART. XXXIX.—*On Hansen's Theory of the physical Constitution of the Moon;* by SIMON NEWCOMB.

THE great reputation of the author has given extensive currency to the hypothesis put forth by Prof. Hansen some years since, that the center of gravity of the moon is considerably farther removed from us than the center of figure. The consequences of this hypothesis are developed in an elaborate mathematical memoir to be found in the twenty-fourth volume of the Memoirs of the Royal Astronomical Society. But the reception of the doctrine seems to have been based rather on faith in its author, than on any critical examination of its logical foundation.* Such an examination it is proposed to give it. An indispensable preliminary to this examination is a clear understanding of what the basis of the doctrine is. Let us then consider these three propositions:

1. The moon revolves on her axis with a uniform motion equal to her mean motion around the earth.

2. Her motion around the earth is not uniform, but she is sometimes ahead of and sometimes behind her mean place, owing both to the elliptic inequality of her motions and to perturbations.

3. Suppose her center of gravity to be farther removed from us than her center of figure, and so placed that when the moon is in her mean position in her orbit, the line joining these centers passes through the center of the earth.

Let us also conceive that these two centers are visible to an observer on the earth. Then a consideration of the geometrical arrangements of the problem will make it clear that when the moon is ahead of her mean place, the observer will see the two centers separated, the one nearest him being farther advanced in the orbit, while, when the moon is behind her mean place, the nearest center will be behind the other. This apparent oscillation of the two centers is, indeed, an immediate effect of the moon's libration in longitude.

Now the inequalities in the moon's motion, computed from the theory of gravitation, are those of a supposed center of gravity. But the inequalities given by observation are those of the center of figure. Hence, in the case supposed, the inequalities of observation will be greater than those of theory.

* In this connection it is curious to notice that on page 83 of his memoir Hansen appears as the first of the independent modern discoverers of Cagnoli's theorem of spherical trigonometry:

$$\cos a \cos b \cos C + \sin a \sin b = \cos A \cos B \cos c + \sin A \sin B.$$

This was about three years before the above formula was published as new by Mr. Cayley, and geometrically demonstrated by Prof. Airy in the Philosophical Magazine.

Also, their ratio will be inversely as that of the distances of the centers which they represent.

Prof. Hansen, in comparing his theory with observations, found that the theoretical inequalities would agree better with observation when multiplied by the constant factor 1·0001544. Supposing that this result could be accounted for on the hypothesis of a separation of the centers of gravity and figure, he thence inferred that the hypothesis was true. But the result cannot be entirely accounted for in this way, because the largest inequality of theory (evection) has a factor (eccentricity) which can only be determined from observation, and therefore even the theoretical evection is that of the center of figure, and not of the center of gravity. It must not be forgotten that the eccentricity, which is not given by theory, is subject to be multiplied by the same factor that multiplies the other inequalities. To be more explicit,

Let e be the true eccentricity of the orbit described by the moon's center of gravity. Then the true evection in the same orbit will be

$$e \times A\,;$$

A being a factor depending principally on the mean motions of the sun and moon. And on Hansen's hypothesis, the *apparent* evection, or that of the center of figure, will be

$$e \times A \times 1{\cdot}0001544.$$

On the same hypothesis, the eccentricity derived from observation, being half the coëfficient of the principal term of the equation of the center, will be

$$e \times 1{\cdot}0001544,$$

and the theoretical evection computed with this eccentricity will be

$$e \times 1{\cdot}0001544 \times A,$$

which is the same with that derived from observation. Hence,

The theoretical evection will agree with that of observation, notwithstanding a separation of the centers of gravity and figure of the moon.

That Hansen overlooked this point is to be attributed to his method of determining the lunar perturbations, by numerical computation from the various elements of the moon's motion, so that the manner in which the inequality depends on the elements does not appear. It is only when we determine the perturbations in algebraic form that this dependence appears.

Passing, now, from the evection, the next great perturbation of the moon's motion is the variation. But the value of this perturbation has not been accurately determined from observation, because, attaining its maxima and minima in the moon's octants, it is complicated with the moon's semi-diameter and parallactic inequality. Even if the semi-diameter is known, the two inequalities in question cannot be determined sepa-

rately with precision, because their coëfficients have the same sign in that part of the moon's orbit where nearly all the meridian observations are made. From this cause Airy's value of the parallactical inequality from all the Greenwich observations from 1750 to 1830 was 3″ in error. And when, in his last investigation,* Airy rejected the observations previous to 1811, owing to some uncertainty as to what semi-diameter should be employed, the result was still a second too small. It is therefore interesting to find what value of the variation will result if we substitute the known value of the parallactic inequality in Airy's equations for the determination of that element. Neglecting those unknowns which have small coëfficients, these equations are, from 1806 to 1851,

1806—15	10·66W	+ 28·14V	=	+ 17·2
16—24	9·45	+ 30·92		+ 24·9
25—33	9·43	+ 29·26		+ 42·1
34—42	9·29	+ 27·28		+ 10·8
43—51	9·05	+ 23·36		+ 7·9
Sum,	47·88W	+138·96V	=	+102·9

In these equations $W\times 0''\!\cdot\!73$ represents the correction to the coëfficient of variation, and $V\times 3''\!\cdot\!77$ that to the coëfficient of parallactic inequality. We now know from recent special investigations that the latter coëfficient is very near $125''\!\cdot\!50$. Airy's provisional one was $122''\!\cdot\!10$, whence,

$$V = \frac{125''\!\cdot\!50-122''\!\cdot\!10}{3''\!\cdot\!77} = 0\cdot 90.$$

The sum of the preceding equations gives

$$W = 2\cdot 15-2\cdot 90V = -0\cdot 46.$$

The resulting correction to the provision variation $(2370''\!\cdot\!3)$ is therefore $-0\cdot 46\times 0''\!\cdot\!73 = -0''\!\cdot\!34$, making the variation derived from observation.........................2369″·96,
while Hansen's theoretical value is................2369 ·86,
and Delaunay's, ...2369 ·74.
The differences are too minute to found any theory upon.

Leaving the evection and variation, the other inequalities are so minute that their product by Hansen's coëfficient is altogether insensible.

Summing up the results of our inquiry, it appears that in the case of the evection the supposed discordance between theory and observation would not follow from Hansen's hypothesis, and therefore, even if it exists, cannot be attributed to that hypothesis as a cause. In the case of the variation no such discordance has been proved. In the case of the other inequalities the discordance would be insensible.

The hypothesis is therefore devoid of logical foundation.

* Memoirs Royal Astronomical Society, vol. xxix.

Art. XL.—*Notices of papers in Physiological Chemistry*—No. II; by George F. Barker, M.D.

5. *On the formation of Sugar in the Liver.*

(1.) In November, 1848, Bernard and Barreswil announced to the French Academy* the discovery of a sugar similar to glucose, as a normal constituent of the liver, and exhibited a specimen of alcohol which they had prepared from it. This sugar, they maintained, could not be recognized in any other organ, although it was constantly present in the liver, even of animals whose food had been for a long time exclusively animal.

(2.) In a second communication, made to the Academy in October, 1850,† Bernard claimed to have demonstrated experimentally, during the two previous years, 1st, the constant presence of sugar in the liver, as a necessary condition of nutrition; 2d, the production of sugar as an especial function of the liver, independent of the particular food taken; and 3d, the dependence of the sugar-forming function upon nervous stimulus. The universality of this saccharine condition of the liver was established by experiments upon the principal classes of vertebrate animals, including man, as well as upon the mollusca among invertebrates. And his conclusion is that the blood which issues from the liver by the hepatic veins during digestion, is invariably saccharine, whatever the nature of the food. The amount of sugar, moreover, which is poured into the general circulation by the hepatic veins, diminishes with the activity of the digestive process; so that after a longer or shorter time, no sugar can be detected, either in the blood of these veins or in the tissue of the liver itself. The second point, the production of sugar by the liver, Bernard proved by feeding animals upon a diet exclusively animal, for four, five, or even eight months; at the end of this time, while no sugar could be detected either in the intestines, or in the blood of the portal vein, the blood of the hepatic veins was strongly saccharine. If therefore, the blood which enters the liver be free from sugar, and that which flows from it be rich in saccharine matter, it is evident that the sugar thus found has been supplied to it by the liver; and hence that this organ has the peculiar function of forming sugar. This sugar has been detected even in fœtal livers; beside being readily fermentable, its solution is browned on being heated with alkaline hydrates, and easily reduces alkaline solutions of copper. Finally, the necessity of nervous

* C. R., xxvii, 514. † Ib., xxxi, 571.

stimulus to the production of sugar by the liver, was demonstrated by showing that this function ceased entirely on dividing the pneumogastric nerves of both sides in the vicinity of the heart.

(3.) The same year, C. G. LEHMANN published* the results of an extended research on the relative composition of portal and hepatic blood, made in order to elucidate the function of the liver. The experiments were made with horses; their food consisted of 2½ lbs. rye-bran, 2 lbs. chopped straw and 2 lbs. hay, and they were killed by injecting air into the jugular vein, 5 or 10 hours after eating. The blood was collected without admixture, and minutely examined. After describing the physical and morphological characters of the two varieties of blood, their chemical differences are considered. Portal blood contains fibrin, normal in amount and in properties; hepatic blood contains none. As to sugar, though abundant in the intestinal canal during the digestion of a mixed diet, Lehmann found that often it could not be detected in portal blood, though sometimes traces were present, and twice he was able to estimate it quantitatively. To examine for it, the alcoholic extract of the residue of the blood, was precipitated by a freshly prepared alcoholic solution of potassic hydrate, the precipitate dissolved in water containing tartaric acid, and examined either by fermentation or a copper-test. In portal blood 0·055 per cent (calculated on the dry residue) of sugar was found, and in the serum ·0052 per cent. In the hepatic veins on the other hand, sugar is not only uniformly present, but in larger amount than in the blood of any other vein. In three experiments, the dried residue of this blood gave 0·635, 0·893, and 0·776 per cent of sugar. In the hepatic blood of a dog fed on flesh, 0·838 per cent of sugar was found. Since therefore, the hepatic vein contains so much more saccharine matter than the portal, there can scarcely be a doubt of the formation of sugar by the liver. The remarkable disappearance of fibrin from the portal blood, may here find an explanation; since the protein bodies may, like salicin and similar substances, split into sugar and some other body containing their nitrogen.

(4.) In a paper published in 1851,† BAUMERT, after enumerating the intimate relations which exist between the different carbo-hydrates, and alluding to their rare occurrence in the animal organism, notices Bernard's investigations, and confirms them by experiments of his own. From 6 pounds of fresh sheep's livers, he obtained 3½ scruples of 70 per cent alcohol,

* Ber. d. K. S. d. Wiss. in Leipzig, 1850; J. pr. Ch., liii, 205.

† Jahresb. d. schles. Gesellsch. f. vaterl. Cultur, vol. xxviii; J. pr. Ch., liv, 57, Nov.

(sp. gr. 0·892); and from the liver of a fox fed for six weeks on flesh, and killed with strychnia, he obtained sufficient alcohol to recognize all its properties. He controverts the opinion that the hepatic sugar is simply retained there from a previous starchy food, since were this the case, the quantity of this sugar found in the herbivorous liver should be much greater than it is. Nor can glucose be carried to the liver during a meat diet, by the portal vein, since he was unable to detect it in portal blood, though his experiments were made with great care. Baumert also confirms Bernard's statement that sugar exists in the blood of the right ventricle, and considers it probable that it is oxydized in the lungs; though he was unable to demonstrate experimentally any of the intermediate products of this oxydation.

(5.) DESSAIGNES in 1854,* showed that the aqueous extract from the liver of the calf contained sugar, because, when mixed with chalk, and kept at a temperature of 25° to 35° C., it actively fermented, and yielded lactic acid, of the same kind as that produced from milk.

(6.) In January, 1855, FIGUIER presented a paper to the Academy† in which he sought to prove the existence normally of sugar in the blood of animals fed on a mixed diet, in amount equal to about one-half that found by Bernard in the liver; and, since the meat on which Bernard's animals were fed, came from the herbivora the blood of which contains sugar, he argues that the liver is an organ designed simply for storing up the sugar which reaches it by the portal vein. The amount of glucose which he found in the *liver* of rabbits was 1·3 per cent, and of cattle 1·4 per cent. In the *blood* of rabbits he found 0·57, in that of an ox 0·48, and in that of man 0·58 per cent. He also calls attention to the fact that the product of the action of the digestive fluids upon the albuminates, called albuminose, prevents the detection of sugar by the copper-tests.

(7.) In February, LONGET published‡ the results of some careful experiments upon the influence exerted by albuminose upon the detection of sugar. He was never able to detect sugar in portal blood by the copper-test, even of animals living on a mixed diet. And he concludes from his research, that when the product of the transformation of a nitrogenous food by the gastric juice exists in the blood in considerable amount, and at the same time glucose in small amount, neither the copper-test, boiling with potassic hydrate, polarimetry, alcoholic fermentation, nor in a word, any other method in use, can demonstrate directly the existence there of the saccharine matter.

* J. pharm. III, xxv, 29; Jahresb., 1854, 405.
† C. R., xl, 228.
‡ Ib., p. 286.

(8.) In March, LEHMANN communicated to the Academy* the results already mentioned, together with some new facts. He found not a trace of sugar in the portal blood of dogs, either when fasting or fed on meat; when fed on boiled potatoes, however, traces were detected. In the hepatic blood, on the other hand, of three dogs fed on meat, 0·814, 0·799, 0·946 per cent of sugar was found; of three others, fasting for two days, 0·764, 0·638, 0·804 per cent; and of two others fed with potatoes, 0·981, 0·854 per cent. In arterial blood, sugar is rarely found. Lehmann was unable to detect it in that of horses fed on starch or oats, or of rabbits fed on sugar, or given a large quantity of beets or carrots.

(9.) To this paper, BERNARD added a note,† in which he says that the meat on which his animals were fed, was proved by analysis to be free from sugar; asserts that the sugar found in the general circulation is liver-sugar which has escaped oxydation in the lungs; controverts Schmidt's idea that sugar, like urea, is formed throughout the body and is simply excreted by the liver; remarks that the formation of sugar by the liver can no longer be disputed; and concludes that the only question remaining is, from what materials is this sugar formed. Since his researches have shown that the ingestion of fat diminishes the production of sugar, he is disposed to agree with Lehmann that it is formed from nitrogenous materials.

(10.) On the 26th of March, FIGUIER communicated‡ new experiments to prove the existence of sugar in portal blood. A young dog, large and vigorous, was fed, after three days fasting, with raw beef for eight days, then allowed to fast forty hours, and then given 2½ pounds of raw beef. Two hours after, an incision was made in the right side of the animal, the index finger introduced through the opening along the inferior border of the liver, and the portal vein seized and ligated. Then the abdomen was opened and the blood from the engorged portal vein collected. By opening the thoracic cavity, blood was obtained from the right ventricle and from the inferior vena cava. The portal blood weighed 102 grams; it was coagulated by the addition of three times its volume of alcohol, and the strained liquid acidulated slightly with acetic acid, was evaporated to dryness, dissolved in water and again evaporated; the residue weighed 1·07 grams. Its solution reacted readily with the copper-test; and by this means the sugar present was determined to be 0·248 per cent of the entire blood. The blood taken above the liver, which weighed 25 grams, left after the above treatment, 0·15 grams, and afforded only traces of sugar. On repeating the experiment with another dog fed for 12 days

* C. R., xl, 585. † Ib., p. 589. ‡ Ib., p. 674.

with raw beef, and examined four hours after taking food, the portal blood contained 0·231 per cent of sugar, the hepatic, 0·304 per cent. Hence the liver simply accumulates the sugar. He contends that his method of operating prevents regurgitation; and to prove it, he examined the portal blood 36 hours after eating, when it contained no sugar, while the liver was decidedly saccharine.

(11.) In a note to the Academy, presented on the second of April,* Bernard says:

"Dans la dernière séance de l'Académie, on a nié l'exactitude de ces faits constatés et vérifiés par les hommes les plus compétents et les plus habiles.

"L'auteur qui a émis cette négation est arrivé non seulement à dire que chez les animaux carnivores, à certains périodes de la digestion, il y a du sucre dans le sang de la veine porte aussi bien que dans celui des veines hépatiques, mais il n'a pas craint d'avancer que deux heures après le repas, on trouve chez un chien qui a mangé de la viande de bœuf crue une plus forte proportion de sucre dans le sang de la veine porte que dans le sang pris au-dessus du foie.

"L'assurance avec laquelle une pareille assertion a été avancée pourrait peut-être en imposer à certaines personnes. C'est pourquoi je crois de mon devoir de venir déclarer ici que ces résultats sont entièrement inexacts." * * * * "Or je déclare de nouveau que j'ai toujours obtenu le résultat que j'avais annoncé, à savoir que chez un chien en digestion de viande cuite ou crue il n'y a pas de sucre dans la veine porte, ni une heure, ni deux heures, ni trois heures, etc., aprés le repas, et qu'il y en a au contraire dans les mêmes circonstances constamment et en notable proportion dans le sang des veines hépatiques."

(12.) At the same session, Bernard presented a note from Lehmann,† confirming his view that the hæmatin which disappears from the blood in considerable quantity in passing through the liver, is converted into sugar. He finds, 1st, that on dry distillation, hæmatin crystals yield at first an acid vapor, like the carbo-hydrates; and 2d, that on reducing the hæmatin dissolved in alcohol by nitrous ether, it yields a product which reduces the copper-tests, and which ferments, giving carbonic acid and alcohol.

(13.) On the 16th of April,‡ Poggiale gave the results of his experiments made to decide the sugar-forming power of the liver, which in the main confirm Bernard's views.

(14.) At this session also, Leconte, one of Majendie's assistants, who had aided Bernard in his investigations, communicated§ the method adopted at the College de France, for demonstrating the sugar-forming function. The animals experi-

* C. R., xl, 716. † Ib., p. 774. ‡ Ib., p. 887. § Ib., p. 903.

mented on were all killed by a section of the medulla; the portal vein was then ligated through an incision on the right side, the abdomen opened, and the inferior vena cava tied below the diaphragm. By cutting through this muscle, a second ligature was placed upon the vena cava, just above it. It was then easy to collect the hepatic blood without admixture, by introducing a glass tube into that portion of the vena cava included between the ligatures. The portal blood was obtained by means of a second tube introduced below the portal ligature. Experiment has shown that blood taken between this ligature and the liver always contains sugar, from regurgitation. The blood to be examined is mixed with three times its weight of strong alcohol, strained through linen, pressed out, the residues and vessels washed with alcohol, the whole filtered, acidulated with a few drops of acetic acid and evaporated on the water bath. The residue is mixed with water, a gram of fresh yeast is added, and the whole is introduced into a graduated bell-jar over mercury, standing in a warm place. A like quantity of the same yeast mixed with distilled water is used as a blank test, for comparison. After from 18 to 24 hours, the gas evolved is measured, the necessary corrections are made, and the amount of sugar calculated. Three careful quantitative examinations made in this way, showed no sugar in the portal blood of animals fed on meat, either raw or cooked; while under the same conditions the hepatic blood contained from one to four thousandths of its weight of sugar.

(15.) In May, 1855, F. W. Pavy communicated to the Royal Society* the results of his researches upon the disappearance of the liver-sugar from the circulation. By following the course of the arterial blood through the capillaries into the veins, he proved experimentally the gradual disappearance of the sugar throughout this course, though not with equal rapidity at all points; the capillaries of the chylopoietic viscera accomplishing it so perfectly that the portal blood is remarkably free from sugar, while that of the other large venous trunks, as the jugular and femoral, is considerably saccharine. Special investigation showed that the destructiou of sugar, which takes place principally in the lungs, is not dependent upon the amount of oxygen supplied there; and farther, that sugar does not disappear unless fibrin be present in the blood, both fibrin and oxygen being necessary, apparently, for its destruction. And inasmuch as the direct oxydation of glucose is not easy, Pavy suggests that under the influence of fibrin acting as a ferment, the sugar is split into lactic acid; an opinion which is strengthened by numerous ingenious experiments.

* Proc. Roy. Soc., vii, 371.

(16.) On the 18th of June, the Commission of the Academy, consisting of PELOUZE, RAYER, and DUMAS, to whom had been referred the papers of Figuier, Poggiale, and Leconte, made their report.* Confining themselves to a verification of facts, they state that since the universal presence of sugar in the liver, as the evidence of an important function of this organ, has never been contested, the questions to be settled, are its origin in this viscus, its use there, and its final disappearance. After enumerating the views of the authors named, the Report says that Bernard's view rests upon four data: 1st, the constant presence of sugar in the liver of all animals; 2d, the presence no less constant, of sugar in the hepatic veins; 3d, the absence of sugar in the portal blood of animals fed on animal food; and 4th, the temporary appearance of sugar in portal blood, when the food is amylaceous or saccharine. Of these points, two, the 1st and 4th, are universally admitted; it therefore remains only to ascertain if the portal blood of animals fed on meat, contains sugar. The Commission were unable to find a trace of sugar in their experiments. And on comparing the portal and the hepatic blood of the same animal, they found that while the portal blood contained not a trace of sugar, that of the hepatic veins contained it in appreciable quantity. They therefore remark, in conclusion; "la doctrine professée par notre confrère paraît intacte."

(17.) On the 27th of August, FIGUIER replied to this Report,† by asserting that the reason why sugar was not detected in the portal blood, was the presence there of some substance which prevented fermentation. He took a large dog which had been fed on horse-flesh for 8 days, supplied it with some of the same meat cooked, and 6½ hours afterward, ligated the portal vein as already described. The blood drawn from this vein, weighed when defibrinated, 700 grams; 600 grams of this were treated with 2½ volumes alcohol, strained from the coagulum, acidulated slightly with acetic acid, and evaporated on the water-bath. The residue was dissolved in water, the solution strained and divided into two portions; one of these was mixed with yeast under suitable conditions for fermentation, but with no result; the other portion was boiled for five minutes with a few drops of nitric acid, the clear yellow liquid exactly neutralized with sodic carbonate and mixed with well washed yeast. In 15 minutes, fermentation began and continued several hours; the gas evolved was absorbed entirely by potassic hydrate, and on distillation, alcohol, recognizable by its odor and its reduction of potassic dichromate with the odor of aldehyd, was

* C. R., xl, 1281. † C. R., xli, 352.

obtained. The experiment was repeated many times and always with the same result.

(18.) In a communication presented by BERNARD* on the 24th of September, he recapitulates the grounds on which he bases his new function of the liver, and reviews Figuier somewhat sharply, accusing him of shifting his ground in each of his three papers; he had himself repeated the experiments given in the last one with no result, no fermentation occurring even after boiling with acid. If, however, sugar be present in the portal blood, fermentation readily takes place, without preliminary treatment. Bernard then passes to the subject of his paper, "the mechanism of the glycogenic function of the liver." After alluding to the view of Schmidt (1849), that sugar is formed in the general circulation, of Lehmann that it is formed in the liver from fibrin or hæmatin, and of Frerichs that nitrogenous matters break up in the liver into urea and sugar—all of which views suppose the change to take place in the blood,—he advances the opinion that the glycogenic substance exists in the tissue of the liver itself. This view he sustains by the following experiment: a vigorous and healthy dog, fed for many days exclusively upon meat, was killed by section of the medulla, seven hours after a full meal of tripe. The abdomen was immediately opened, the liver carefully removed, and while yet warm, before coagulation of the blood had taken place, the portal vein was connected with the laboratory hydrant by means of a rubber tube, and water allowed to flow through the organ, escaping from the hepatic veins in a strong jet. Gradually the tissue of the liver became paler, and in 15 minutes the water was colorless. After 40 minutes, the escaping water contained not a trace either of sugar or of albuminoid matter; and on macerating a fragment of the viscus, no sugar was detected in the tissue. The liver was then allowed to remain exposed to the air for 24 hours, after which time sugar existed in it abundantly; even a little liquid remaining in the dish gave the reactions; and on injecting water into the portal vein, it issued strongly saccharine. The liver must therefore contain normally two substances: first sugar, very soluble in water, and easily removed by washing; and second, another body, sufficiently insoluble to remain after 40 minutes washing. This it is, which, when the liver is left to itself, changes into sugar. The change is prevented by cooking, and facilitated by chopping the organ fine; it is generally completed within 24 hours, the liver then containing as much sugar as at first; but a second fermentation is not possible, the material being exhausted. Bernard, moreover, proved the insolubility of this sugar-forming substance, in alcohol and ether; and showed that it is confined strictly to

* C. R., xli, 461.

the liver, not being recognizable in portal or in any other blood. It is absent from this viscus, whenever the conditions are such as to interfere with the production of sugar. This substance renewed incessantly under the influence of nutrition, is continually being transformed during life into saccharine matter, which replaces in the liver that which is constantly removed by flow of blood, into the hepatic veins. After death, the change continues until the material is exhausted; but then as the sugar is not removed, it accumulates in the liver-tissue; this is so charged with it, that it contains a considerably larger proportion of sugar the next day, than is found when the animal is killed.

(19, 20.) A second research by LEHMANN upon "the constitution of the blood of various vessels, with especial reference to the sugar it contains," was published during this year,* a portion of which was presented to the Academy Oct. 22d,† by Bernard. The experiments were made on the blood of five horses, by the methods already given, the analysis being made in duplicate. In the blood of the inferior vena cava, in three trials, 0·346, 0·311 and 0·492 per cent of sugar was found; but none could be detected in that of the jugular, external abdominal, or cephalic veins, nor in that of any artery. Though the uniformity of the analytical results ought to settle the question of the production of sugar by the liver, yet, as it has been strenuously denied, Lehmann thinks it of importance to examine the methods both chemical and physiological, which have been used. Of the former, the copper-test, either of Trommer, or as modified by Fehling, even when the materials used are pure, cannot be relied on for the detection of minute quantities of glucose in mixed fluids, since the precise action of all the substances present is not known. Hence Figuier's results, obtained with Fehling's solution in an alcoholic extract of portal blood, were not sustained by the Academy Commission, using the fermentation test. Longet's statement that albuminose (the peptone of Lehmann) prevents the copper reaction, is only partially true; since when sugar and albuminose are both present, the cupric oxyd is reduced, the blue solution changing to yellow, but with the formation of no precipitate. That albuminose hinders fermentation, is true only when the solution is so strong, that a similar solution of sugar would do the same. No true chemist, however, would think of testing directly a fluid containing much albuminose. German chemists, at least, use alcoholic extracts of these fluids, in which but a trace of albuminose exists. But, even in this, it is conceivable that there may be substances which will reduce the copper-test; since 1840, therefore, the sugar has been precipitated from the alcoholic extract for examination by

* Ber. d. K. S. Ges. d. Wiss. in Leipzig; J. pr. Ch., lxvii, 321. † C. R., xli, 661.

alcoholic potash. No substance which can interfere with the tests is thus thrown down, so that the copper-test may be directly used, or the solution, slightly acidified with tartaric acid, may be at once fermented. This method is one of great delicacy; sugar has been detected by it in urine, to which only $\frac{1}{100,000}$ was added; and in many fluids where the reaction, as ordinarily applied, was doubtful or failed entirely, such as the urine of arthritis and tuberculosis, sugar was readily detected by this process. Moreover, glucose has thus been discovered in egg-albumin, in mammalian ovaries, and sometimes in mixed venous blood; but never in the bile or the saliva. As to the physiological method, it is evident that it should furnish blood such as flows in the veins during life. This is especially necessary with portal blood, as at death there is a stasis of blood in the liver; and, since the nutrient arteries of the liver pour their blood, not into the hepatic vein, but directly into the portal capillaries, there may easily be regurgitation of blood containing sugar. Though Figuier has avoided this source of error, he has fallen into another; it needs no deep insight into the distribution of the blood to perceive that the 400 grams of portal blood which he took is far more than the normal content of that vein, even in a very large dog. According to the analysis of Bischoff and Welker, the blood constitutes not more than $\frac{1}{13}$ of the bodily weight of an animal; in a dog of 24 kilograms, there cannot be more than 2 kilograms of blood. To suppose that the 400 to 700 grams taken from the portal vein of such an animal is pure portal blood, is to make the absurd assumption that $\frac{1}{4}$ of all the blood is found in the portal system. To avoid these errors, Lehmann proceeds as follows: The dog is killed by a blow on the head, the portal vein tied through a small opening, as Bernard suggests, the abdominal cavity opened quickly, a noose placed 10 or 15 m.m. below the ligature, the blood pressed toward the mesenteric and splenic veins, an opening into the empty vein made with scissors, a glass tube bent twice at right angles inserted, the noose drawn tight about the tube, and the blood allowed to flow through it. In this way, 213 grams of blood were collected; but this was too much, as analysis proved. Within four months, 32 dogs were operated on in this way; after fasting for 24 hours, they were fed with raw horse-flesh, and killed 3, 4, 5, or 6 hours subsequently. In 16 of these, from whom from 35 to 81 grams of portal blood were taken, not a trace of sugar was detected. In order to see whether any difference would result from taking a larger quantity from the living animal, 351 grams were taken from a dog weighing 13 kilos., 211 grams from one of 11·5 kilos., and 263 grams from a third weighing

14·5 kilos.; and in all of these specimens, as in that of 213 grams above mentioned, sugar was found; thus proving foreign admixture in the blood examined by Figuier. Lest it be said that the quantity examined was insufficient, Lehmann united the blood of six dogs in two portions; that of the first three weighed 217·5 grams; that of the second three, 192·7 grams; the result was the same. Experiments made to ascertain whether the portal vein contained any substance capable of yielding sugar, were entirely without result; as also were those made to discover the presence of any material which prevented the reactions for sugar.

(21.) At the conclusion of this paper, Bernard remarked that the glycogenic function of the liver, as at first enunciated by him, was now incontestibly proven.

(22.) In a paper published by HENSEN,* he confirmed Bernard's view that the liver contains an insoluble body which is converted into sugar by the action of a ferment, by acting on the substance of the boiled liver by the salivary and pancreatic ferments, and thus producing sugar; he also converted starch into sugar by means of the liver ferment.

(23.) POGGIALE, in a communication to the Academy in Feb. 1856,† contests the opinion that the presence of an alkaline carbonate in the blood is necessary to the destruction of sugar, and hence that its appearance in diabetic urine is due to a deficiency in this respect; since he finds that the addition of hydro-sodic carbonate to the food of dogs produces no effect on the amount of sugar in the blood; and that sugar when injected into the blood, appears equally in the urine whether dissolved in pure water or mixed with twice its weight of the carbonate.

(24.) On the 26th of May, 1856, CHAUVEAU published‡ the following results: 1st, during very prolonged abstinence, sugar never disappears from the general circulation; 4 horses and 4 dogs fasting from 12 hours to 6 days, had in their venous blood from ·066 to ·080 per cent of sugar for the former, and ·029 to ·034 per cent for the latter; in the arterial blood, from ·073 to ·093 per cent for the horses, and ·035 to ·053 for the dogs: 2d, sugar is more abundant in the arteries than in the corresponding veins: 3d, arterial blood wherever taken, contains the same amount of glucose; that examined was from the coccygian, femoral and carotid arteries, and from the left ventricle: 4th, venous blood, except portal during the digestion of starchy or saccharine matters, hepatic, and that of the inferior vena cava above the diaphragm, shows no appreciable variations in

* Verh. d. phys.-med. Ges. zu Würzburg, vii, 219; Jahresb., ix, 705.

† C. R., xlii, 198.

‡ Ib., p. 1008.

the amount of glucose: 5th, with animals fasting or fed on meat, hepatic blood is more saccharine than any other: 6th, the quantity of sugar is the same in both sides of the heart: 7th, pure lymph is always saccharine, even after prolonged abstinence: and 8th, this sugar is not absorbed from the tissues by the lymphatics, since no tissue except the liver contains it. His conclusions are as follows: 1st, herbivorous blood contains more sugar than carnivorous: 2d, the sugar in the right side of the heart is not destroyed in the lungs and hence appears in the left side: 3d, a certain quantity disappears in the capillaries, the returning blood containing less than the issuing: 4th, the sugar is not fixed by the solids of the body; a part filters to the lymphatics, and a part is metamorphosed; 5th, the blood approaching the right side of the heart has its sugar increased by that in the chyle: 6th, sugar is re-supplied to the blood by the hepatic veins: 7th, the sugar thus furnished does not exist in the portal blood of animals, either fasting or fed with meat; it comes from the liver, which is therefore a true producer of sugar.

[*To be continued.*]

ART. XLI.—*Observations of the Auroras of Sept. 5th and 15th, 1868, at Mt. Desert Island, Maine* (lat. 44° 20′ N., long. 68° 15′ W.); by W. S. GILMAN, Jr.

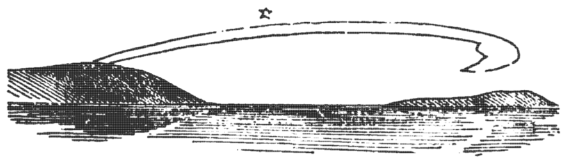
ON the evening of Sept. 5th, 1868, there was a fine aurora here. At 8 P. M. streamers shot up in the north and N.N.E.; one large ray attained an altitude of about 50°. These streamers moved toward the west. At 9^h 15^m a long thin cloud-like arc spanned the heavens from east to west, passing to the north of ζ Pegasi, γ Delphini, γ Sagittæ and α Ophiuchi. Near the meridian its breadth was about a degree, and it was broken into short parallel bands of light.

At 9^h 30^m the arc of light was more to the south, bordering on the Dolphin and Arrow. At 10^h 30^m aurora faintly visible under Polaris.

On the evening of Sept. 15th, from 7^h until 11^h 30^m, there was a remarkably fine display of the aurora here. On the 14th, from 9 to 10 A. M., I had noticed dusky beams in the N.N.E. not unlike those proceeding from a very regular night aurora. They did not appear further west than the north point, and rose above the horizon about 10°. No aurora was marked during the evening of the 14th.

On the 15th, at 7 P. M., several short beams in the N.N.E. moving slowly westward. At 7^h 25^m an auroral arc from Algol

to Capella less than a degree broad, quite bright, and shaped like a whale's rib. The right end curves downward toward the horizon, as shown in the accompanying sketch.



At 7^h 35^m a second luminous arc formed about half way to Algol.

At 7^h 42^m brilliant beams between Capella and Polaris.

7^h 55^m. Brightest part of arc has shifted to westward, and is directly under the Bear's head. Sheets of light going up from Polaris west.

8^h 15^m. The arc is curiously shaped; the upper edge is like the outline of a huge mountain. To the east the arc drops down toward the horizon and ends in a twisted cloud of light.

8^h 55^m. Auroral cloud spans the heavens. Origin in east about 8° above horizon, between Jupiter and α Trianguli. Moved slowly southward.

9^h 0^m. The highest part of arch is now at β Cygni.

10^h 20^m. Streamers in sheets—lower edge foliated like the plaits in a garment.

10^h 30^m. The north appears covered with great rolls of light like so many huge rolls of cloth. The sheets of light appear to be rolling and unrolling from E. to W. and from W. to E. To the west the sheets seem to disappear around the edge of a circle, as if part of concentric cylinders of light suspended above the pole of the earth. The folds bear a striking resemblance to a gauze curtain agitated by a strong wind.

10^h 45^m. The twisted beams began to flicker from the horizon upward like vast flames from a great conflagration. There were two or three flashes to a second. The flames passed the zenith and went over to the western horizon. The heavens now appeared all afire to the north and up to the zenith, the flames ascending from the entire northern horizon from the E. to the W. point.

11^h 0^m. The flickering can be seen to within 8° or 10° of Fomalhaut in the south.

11^h 10^m. Flashings cover more than half the sky to within about 20° of southern horizon.

11^h 30^m. Flashing light still continues but is growing much less distinct. Heavy mass of clouds obscured the northern sky under Polaris.

[The preceding notes are extracted from a very minute report

of these auroras furnished us by Mr. Gilman. If any other observer, favorably situated, has carefully noted the same phenomena, and will communicate his observations to us, we will institute a comparison of the observations, and make a report of the results. In order to determine the height of these auroral exhibitions, it is desirable to obtain simultaneous observations at two places situated near the same magnetic meridian, and at a distance from each other of about 100 miles.—Eds.]

Art. XLII.—*Discoveries of new Planets.* (Communicated by Prof. J. C. Watson of Ann Arbor.)

Planet (103).—The following are my observations on the new planet (103) discovered by me on Sept. 7th:

Ann Arbor M. T.	(103) α	(103) δ	Comp.
1868. Sept. 7, 15^h 31^m 46^s	0^h 22^m $13^s{\cdot}18$	—3° 49′ 51″·3	9
9, 14 45 42	0 20 53 ·94	4 3 47 ·0	1
10, 9 59 59	0 20 20 ·66	—4 9 24 ·5	7

The planet is of the 10th magnitude.

New planet (104); (communicated Sept. 14th).—I have the pleasure to send you the following observations of a new planet which I discovered last night:

Ann Arbor M. T.	α	δ	Comp.	*
1868. Sept. 13, 11^h 35^m 9^s	0^h 20^m $25^s{\cdot}61$		2	*a*
13, 12 35 52	0 20 23 ·65	—1° 10′ 52″·6	4	*b*
13, 13 1 33	0 20 22 ·56	1 10 48 ·0	2	*c*
13, 15 55 57	0 20 17 ·38	—1 11 23 ·7	10	*c*

Daily motion, $\Delta\alpha = -45^s$ $\Delta\delta = -5'$.

The planet is of the 11·5 magnitude.

Planets (102) *and* (105); (communicated Sept. 18th).—On the 23d of August, Dr. C. H. F. Peters of Clinton, N. Y., discovered minor planet (102). He proposes to call it ***Miriam***, and has communicated the following observations:

Clinton M. T.	(102) α	(102) δ
1868. Sept. 13, 12^h 48^m 30^s	1^h 14^m $1^s{\cdot}93$	+12° 8′ 50″·3
14, 13 17 23	1 13 40 ·13	12 3 57 ·1

Planet 11th magnitude.

The discoveries of minor planets (103) and (104) have already been communicated to you. I have yet to add that I discovered still another planet on the 16th inst., of which I have observed the following places:

Ann Arbor M. T.	(105) α	(105) δ
1868. Sept. 16, 16^h 3^m 15^s	0^h 13^m $47^s{\cdot}42$	+6° 12′ 4″·9
16, 16 33 22	0 13 46 ·28	6 11 45 ·3
17, 10 29 16	0 13 10 ·57	+6 1 7 ·2

The planet is somewhat brighter than a star of the twelfth magnitude.

Discovery of Planet (106), *and new observations on* (100), (101), (102), (103), (104), (105); (communicated Oct. 13th).—I have observed the following places of a new planet discovered by me on the 10th inst.:

Ann Arbor M. T.	(106) α	(106) δ
1868. Oct. 10, $10^h\ 36^m\ 37^s$	$1^h\ 1^m\ 21^s{\cdot}84$	$+0°\ 31'\ 42''{\cdot}5$
11, 11 0 19	1 0 34 ·33	0 28 31 ·0
12, 10 26 52	0 59 48 ·72	+0 25 31 ·2

The planet resembles a star of the tenth magnitude.

The following are my latest observations of the other new planets:

	Hecate (100).	
Ann Arbor M. T.	α	δ
1868. Oct. 11, $8^h\ 31^m\ 58^s$	$20^h\ 42^m\ 59^s{\cdot}68$	$-21°\ 0'\ 32''{\cdot}4$
12, 7 26 57	20 43 36 ·02	−20 59 9 ·6
	Helena (101)	
Oct. 11, 10 12 55	23 5 18 ·35	− 0 24 38 ·6
12, 8 2 0	23 4 50 ·75	− 0 24 2 ·4
	Minor Planet (102), [*Peters.*]	
Oct. 11, 11 54 10	0 57 22 ·67	+ 8 43 46 ·4
12, 10 53 37	0 56 42 ·78	+ 8 35 15 ·7
	Minor Planet (103).	
Oct. 11, 14 32 19	23 57 6 ·49	− 7 24 29 ·6
12, 9 59 13	23 56 36 ·69	− 7 27 52 ·2
	Minor Planet (104).	
Oct. 11, 13 1 49	23 59 34 ·97	− 3 0 22 ·9
12, 9 2 15	23 59 1 ·45	− 3 2 52 ·1
	Minor Planet (105).	
Oct. 11, 12 29 56	23 54 45 ·95	+ 0 16 50 ·8
12, 8 32 37	23 54 14 ·83	+ 0 6 2 ·3

Elements of (100) *and* (101); (communicated Oct. 26th).—I have determined the elements of the orbits of two of these planets, and have obtained the following results:

Hecate (100).		*Helena* (101).	
Epoch = 1868, Sept. 1·0 Washin'n M. T.		Epoch = 1868, Sept. 13·5 Washin'n M. T.	
M = 10° 5′ 30″·4		M = 17° 48′ 53″·0	
π = 304 45 0 ·5	Ecliptic and	π = 328 40 51 ·0	Ecliptic and
☊ = 128 28 37 ·7	mean equinox	☊ = 343 35 0 ·1	mean equinox
i = 6 33 34 ·6	1868·0.	i = 10 4 19 ·5	1868·0.
ϕ = 8 39 32 ·5		ϕ = 8 0 48 ·1	
log a = 0·493331		log a = 0·410460	
log μ = 2·810010		log μ = 2·934317	
μ = 645″·669		μ = 859″·640	

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the combustion of hydrogen and carbonic oxyd in oxygen under great pressure.*—In a communication made to the Royal Society in 1861, Dr. FRANKLAND pointed out the remarkable law that in combustion the diminution in illuminating power is directly proportional to the diminution in atmospheric pressure. The same writer has now renewed the subject, and has presented the results of his study of the influence of an increase of pressure. Dr. Frankland in the first place calls attention to the fact that the theory of Davy, now commonly received, that the illuminating power of a flame depends upon the deposition and ignition of solid particles in the flame, does not hold good for numerous cases. Thus arsenic burns with great brilliancy in oxygen, forming arsenous oxyd, yet arsenic is volatile at 180° C. and arsenous oxyd at 218° C., while the temperature of the incandescence of solids is at least 500° C. A mixture of the vapor of carbonic disulphid with oxygen burns with an extremely brilliant light, but no presence of solid particles can be admitted. The result is the same when nitric oxyd is substituted for oxygen, the flame being then very rich in the more refrangible rays. The combustion of phosphorus in oxygen furnishes another case in point, since phosphoric anhydrid is volatile at a red heat.

The author's experiments on the combustion of hydrogen and carbonic oxyd in oxygen were conducted in a strong iron vessel furnished with a thick plate of glass to permit of optical examination of the flame. The feeble light emitted by hydrogen burning under the ordinary pressure is well known. An increase to two atmospheres produces a visible augmentation, while at a pressure of ten atmospheres the light of a jet one inch long is sufficient to enable the observer to read a newspaper at a distance of two feet from the flame. The spectrum of this flame is bright and continuous from red to violet. The flame of carbonic oxyd in oxygen is more luminous under a pressure of ten atmospheres than the hydrogen flame under the same pressure. When burnt in oxygen under a pressure of fourteen atmospheres the spectrum of the flame is very brilliant and perfectly continuous. When the electric spark is passed through different gases the light is more intense in the cases of those gases which have the greater density. It is particularly brilliant when passed through a tube containing liquid sulphurous acid. If a stream of induction sparks be passed through air condensed in a glass tube by a pump, a marked increase in the light will be observed as the pressure of the air is increased, and the phenomenon occurs in a reverse order when the condensed air is allowed to escape.

The electric arc from fifty Grove's cells is incomparably more brilliant in the vapor of mercury than in air. The author remarks

that these results have a very direct bearing on the views now held regarding the constitution of the stars and nebulæ, and promises to make the application when his complete memoir is laid before the Royal Society.—*Proc. Royal Society*, xvi, 419.

W. G.

2. *On the reduction and saturation of organic compounds by hydrogen.*—Since the publication of our abstract of BERTHELOT'S first three papers on this subject we have received another portion of his complete memoir, from which we extract the following as the most salient points:

(1) Complex and polymeric carbids.

1st section—complex carbids. Allyl or allyl propylene, $C_{12}H_{10}$, heated with a solution of iodhydric acid saturated in the cold gives a new liquid hydrocarbon and a gas in small quantity. The formation of these substances is represented by the equations

$$\underset{\text{Allyl.}}{C_6H_4(C_6H_6)}+2H_2 = \underset{\text{Hydruret of dipropylene.}}{C_6H_6(C_6H_8)}$$

$$C_6H_4(C_6H_6)+3H_2 = \underset{\text{Hydruret of propylene.}}{2C_6H_8}$$

2d section—complex carbids of the aromatic series.

Phenyl, $C_{24}H_{10}$, by taking up hydrogen under the action of an excess of iodhydric acid yields hydrid of hexylene as principal product:

$$C_{24}H_{10}+9H_2 = 2C_{12}H_{14}.$$

With less iodhydric acid it yields as chief product benzine:

$$C_{24}H_{10}+H_2 = 2C_{12}H_6,$$

and as secondary products carbon, hydrid of propylene and free hydrogen.

Styrolene yields hydrid of octylene as chief product:

$$C_{16}H_8+5H_2 = C_{16}H_{18},$$

and less abundantly the hydrids of hexylene and ethylene:

$$C_{16}H_8+6H_2 = C_{12}H_{14}+C_4H_6.$$

With a less quantity of the hydracid, styrolene yields hydrid of styrolene:

$$C_{16}H_8+H_2 = C_{16}H_{10},$$

and less abundantly benzol and hydrid of ethylene:

$$C_{16}H_8+2H_2 = C_{12}H_6+C_4H_6.$$

Ethyl benzine, $C_{16}H_{10}$ or $C_{12}H_5(C_4H_5)$, gives as chief product hydrid of octylene:

$$C_{16}H_{10}+4H_2 = C_{16}H_{18},$$

this last being perhaps hydrid of ethyl-hexylene, $C_{12}H_{13}(C_4H_5)$. In smaller proportion the author obtained also the hydrids of hexylene and ethylene.

3d section—complex pyrogenic carbids.

Naphthalin, $C_{20}H_8$, treated by iodhydric acid yields successively the following products: 1st hydrid of naphthalin, $C_{20}H_{10}$; 2d hydrid (?) $C_{20}H_{12}$, diethyl benzine, $C_{20}H_{14}$, hydrid of diethyl

benzine, $C_{20}H_{22}$, ethyl benzine and hydrid of ethylene, $C_{18}H_{10}+C_4H_6$, hydrid of ethyl-hexylene and hydrid of ethylene, $C_{16}H_{18}+C_4H_6$, benzine and hydrid of ethylene, $C_{12}H_6+2C_4H_6$, hydrid of hexylene and hydrid of ethylene, $C_{12}H_{14}+2C_4H_6$. Of these the normal or complete product is hydrid of decylene, $C_{20}H_{22}$.

Perchlorinated naphthalin, $C_{20}H_8$, yields as chief product hydrid of ethylhexylene, the reaction being

$$C_{20}Cl_8+32HI = C_{16}H_{18}+C_4H_6+8HCl+16I_2.$$

Anthracene, $C_{28}H_{10}$, yields as chief product hydrid of tetradecylene:

$$C_{28}H_{10}+10H_2 = C_{28}H_{30};$$

a notable quantity of hydrid of heptylene and smaller quantities of the hydrids of hexylene and ethylene. With an insufficient quantity of hydracid, anthracene gives toluene, $C_{14}H_8$, traces of benzine and hydrid of ethylene, and a small quantity of what may be hydrid of anthracene, $C_{28}H_{12}$.

Alizarine, $C_{28}H_8O_8$, in presence of a large excess of hydracid is completely changed into saturated carbids, among which are the hydrids of octylene and hexylene.

Phthalic acid, $C_{16}H_6O_8$, yields as principal product hydrid of heptylene, $C_{14}H_{16}$, and a small quantity of hydrid of octylene, the last being the normal product:

$$C_{16}H_6O_8+10H_2 = C_{16}H_{18}+4H_2O_2.$$

Terephthalic acid, $C_{16}H_6O_8$, yields hydrid of heptylene:

$$C_{16}H_6O_8+7H_2 = C_{14}H_{16}+C_2O_4+2H_2O_2.$$

—*Bull. de la Societe Chimique de Paris*, 1868, p. 265. W. G.

3. *On the persulphid of hydrogen.*—The constitution of persulphid of hydrogen has hitherto remained doubtful. Dr. A. W. Hofmann has taken advantage of the fact that great quantities of this substance are produced in certain technical processes at Dieuze to examine its constitution anew, and has succeeded in rendering it certain that there is a persulphid having the formula H_2S_3. When a cold saturated solution of strychnine in strong alcohol is added to an alcoholic solution of persulphid of ammonium, brilliant crystalline spangles soon appear, and after 12 hours beautiful orange-red needles are formed, which after washing with cold alcohol are perfectly pure. They are insoluble in water, alcohol, ether and bisulphid of carbon; in fact, the author has found no solvent from which they can be recrystallized. Analysis leads to the formula

$$\text{Ͼ}_{21}H_{24}N_2\text{Θ}_2S_3 = \text{Ͼ}_{21}H_{22}N_2\text{Θ}_2+H_2S_3.$$

In contact with sulphuric acid the crystals are decolorized, and on adding water colorless transparent oily drops of persulphid of hydrogen are separated, which after some time are resolved into sulphur and sulphuretted hydrogen. Quinine, cinchonine, benzine and some other alkaloids gave no analogous compounds.—*Proc. Royal Society*, xvi, p. 437. W. G.

4. *On the compounds of molybdic and phosphoric acids.*—DEBRAY has discovered and partly described a remarkable series of bodies formed by the combination of phosphoric and molybdic acids, and closely related in general character to the silico-tungstates of Marignac. The yellow precipitate which molybdate of ammonia forms in a solution of phosphoric acid, or a phosphate rendered acid by nitric acid, when boiled in nitro-muriatic acid, gives a yellow liquid which on evaporation yields beautiful doubly oblique prisms containing one equivalent (old style) of anhydrous phosphoric acid and 20 equivalents of anhydrous molybdic acid, together with about 13·3 per cent of water. Two other hydrates exist, of which one, crystallizing in large regular octahedra, contains 23·4 per cent of water, while the other, crystallizing in rhombic prisms, contains 19·6 per cent of water. The quantity of phosphoric acid in these compounds amounts to only 3·7 to 4·1 per cent, yet it modifies the properties of the molybdic acid in a very remarkable manner. Phosphomolybdic acid precipitates from strongly acid solutions potash, the oxyds of cæsium, rubidium and thallium, ammonia and the organic alkaloids. Sodium and lithium give no precipitates under these circumstances; the same is the case with the heavy metallic oxyds when the solutions are sufficiently acid. The yellow phosphomolybdates of potassium, thallium and ammonium have the general formula

$$3RO \,.\, PO_5 \,.\, 20MO_3.$$

The potassic and ammonic salts contain 3 equivalents of water. A solution of phosphomolybdic acid precipitates argentic nitrate; the precipitate gradually becomes crystalline and has the formula

$$7AgO \,.\, PO_5,\ 20MO_3+24HO.$$

Dilute nitric acid dissolves this salt; from the solution crystals are formed having the formula

$$2AgO \,.\, PO_4 \,.\, 20MO_3+7HO.$$

The phosphomolybdates are only stable in the presence of acids; alkalies transform them into ordinary molybdates and phosphomolybdates, in which the two acids are united in the ratio of 1 to 5:

$$PO_5,\ 20MO_3+aq = PO_5 \,.\, 5MO_3,\ aq+15MO_3.$$

These phosphomolybdates are colorless or nearly so and have a nacreous lustre; they are soluble in water and crystallize easily. An excess of acid brings them back to the condition of the yellow phosphomolybdates, setting free phosphoric acid:

$$4(PO_5,\ 5MO_3+10aq = 3(PO_5,\ 3HO)+PO_5,\ 20MO_3+aq.$$

The following are some of the most beautiful salts of this second class of phosphomolybdates:

$$6(NH_4O),\ 2PO_5,\ 10MO_3+14HO$$
$$6KO \,.\, 2PO_5 \,.\, 10MO_3+14HO$$
$$6NaO \,.\, 2PO_5 \,.\, 10MO_3+28HO$$
$$6AgO \,.\, 2PO_5 \,.\, 10MO_3+14HO.$$

These formulas are not divisible by *two*, as the salts in question yield by careful treatment with acids a new series of crystalline salts, having the general formula

$$5RO . 2PO_5, 10MO_3+Aq.$$

A double salt of one of the above acids with potassic nitrate has the formula

$$6KO . MO_5+6KO . 2PO_5 . 10MO_3+18HO.$$

In conclusion, the author gives a new method of analysis suitable to the above-mentioned compounds, for which, however, we refer to the original memoir.—*Comptes Rendus*, lxvi, 702. W. G.

5. *On iodid of silicon and silico-iodoform.*—FRIEDEL has obtained the iodid of silicon, SiI_4, by passing the vapor of iodine mixed with dry carbonic acid gas over silicon heated to redness. After purification the iodid forms colorless transparent octahedral crystals, fusing at 120°·5 C., and boiling at about 290° C. Water decomposes it, forming silicon and iodhydric acid. The density of its vapor was found to be 19·12, theory requiring 18·56. Alcohol acts upon iodid of silicon to form silica and iodid of ethyl according to the equation

$$SiI_4+2C_2H_5O = SiO_2+2C_2H_5I+2HI.$$

The substance obtained by Buff and Wöhler by the action of iodhydric acid upon silicon was found, as in the analogous case of the action of chlorhydric acid, to consist of a mixture of silico-iodoform, $SiHI_3$, and iodid of silicon. The former is, when pure, a very heavy colorless liquid, highly refractive, and boiling at about 220° C. Its density is 3·362 at 0° C., and 3·314 at 20° C., without correction for the dilatation of the glass, so that it is almost as heavy as the thallic alcohol of Lamy.—*Comptes Rendus*, lxvii, p. 98. W. G.

6. *On the density of the vapor of calomel.*—The density of the vapor of calomel was determined by MITSCHERLICH, and more recently by H. SAINTE-CLAIRE DEVILLE and TROOST, who found for it the number 8·21, which agrees with the theoretical number 8·51, which we obtain if we suppose that the formula HgCl corresponds to 4 vols. (H=1 representing 2 vols). Those chemists who represent calomel by the formula Hg_2Cl, suppose that at the temperature at which the vapor-density is taken the vapor is resolved into a mixture of Hg and HgCl, each of which occupies 4 vols. Debray has found, however, that at the temperature of 440° C., at which the vapor-density is taken, a plate of gold plunged into the vapor preserves its color and luster, and is not amalgamated. On the other hand, a plate of gold plunged into the vapor of biniodid of mercury at a temperature near to redness becomes so brittle that it may be rubbed to pieces between the fingers. From this it follows that calomel must be classed among substances which correspond to 8 vols. of vapor.—*Comptes Rendus*, lxvi, p. 1339.

W. G.

7. *Connotations of Magnetism;* by PLINY EARLE CHASE.—The following summary gives the principal points of the generally accepted theory of terrestrial magnetism, together with such modifications as seem to be warranted by the foregoing considerations. Although I can hardly imagine the possibility of essential change

in any particular, I submit it as a merely provisional statement, subject to such future amendments as may be required by the progress of discovery.

1. The earth is an electro-magnet, magnetized by currents which are excited by the sun and by the earth's rotation.

2. Terrestrial magnetism is subject to a variety of disturbances, some of which are periodical, others irregular and occasional.

3. The principal periodical disturbances vary: 1st, with the solar hour; 2d, with the season of the year; 3d, with the relative distance of the earth from the sun; 4th, with the rapidity of the earth's orbital motion; 5th, with the rapidity of change in solar declination; 6th, with the absolute hour, in reference to magnetic meridians in the Pacific and Atlantic oceans; 7th, with the lunar hour; 8th, with the lunar declination; 9th, with the position of the principal planets (sun-spot period); 10th, with change of climate (secular variation). All these disturbances (the last, perhaps, excepted) appear to be transmitted with the speed of gravity, and they may all be grouped under two heads; 1st, change of relative position; 2d, change of relative velocity between the disturbing and the disturbed body.

4. The principal occasional, or irregular disturbances, seem to be dependent upon, 1st, irregular variations in the light or heat transmitted from the sun; 2d, similar variations in the diathermancy or transparency of the earth's atmosphere; 3d, local accumulations of heat or cold; 4th, electric and other local meteoric changes; 5th, atmospheric or terrestrial agitation (cyclones, earthquakes, volcanic eruptions, &c.). These disturbances are, of course, transmitted with velocities varying from that of light or of electricity to that of sound-waves, or even to that of still more sluggish vibrations.

5. The periodical and the occasional disturbances are all modified by mutual interaction. The phenomena are thus complicated, and the difficulty of satisfactory investigation is largely increased.

6. The sources of disturbance are, therefore, multiform; some are celestial, and others terrene; some cosmical, and others local in their origin. They all, however, have this common feature: each is capable of occasioning fluid currents.

7. Every disturbance of terrestrial magnetism is accompanied by a disturbance of terrestrial gravitation.

8. The gravitating atmospheric currents, which are originated simultaneously with the magnetic disturbances, become, in their turn, the source of secondary disturbances.

9. However the disturbance is produced, be it primary or secondary, periodical or occasional, there is an immediate tendency to equilibrium, and that tendency is in the direction of the terrestrial gravitation of fluids.

10. The regular disturbances by the sun's heat and attraction, combined with the rotation and attraction of the earth, produce revolving currents analogous in form to those which circulate around ordinary magnets.

11. Similar revolving currents must be excited about the sun, and about every other rotating celestial body. Such bodies, therefore, may become, like the earth, electro-magnets, or the seats of a specific magnetism.

12. If the specific magnetism is to be measured by the intensity of the exciting disturbance, the sun may be, relatively, the weakest magnet in our system; if it is to be measured by the intensity of the equilibrating force, it is probably proportioned to the force of gravitation at the surface of the magnetized orb.

13. It seems probable that the specific cosmical magnetism, like that of a permanent magnet or of an ordinary electro-magnet, reacts inductively. But I know of no good reasons for supposing that such induction is other than a comparatively unimportant, secondary and subordinate action.

14. The lunar primary disturbances, so far as has hitherto been ascertained, appear to belong exclusively to the first class of periodical disturbances, those which are due to change of relative position. It seems most likely, however, that there may also be lunar periodical disturbances of the second class.

15. The resemblance of the lunar diurnal magnetic curves to the normal tidal curve indicates a close correspondence between lunar magnetism and lunar gravitation.

16. Magnetism does not seem to be, strictly speaking, a simple or independent force, but, like the central force of an eddy, the resultant of revolving currents, moving with a speed which is, probably, at least as great as that of light at the surface of the magnetized body.

17. Terrestrial and cosmical magnetism can be satisfactorily accounted for by the hypothesis that all the magnetic eddies are produced by the action of gravity in the restoration of disturbed equilibrium. No known force, other than gravity, has a known velocity sufficient to enable it to act promptly, as an equilibrating force, in all the known cases of magnetic disturbance.—(*Proc. Amer. Phil. Soc., Feb.* 21, 1868.)

II. MINERALOGY AND GEOLOGY.

1. *On a new Mineral in Cryolite;* by THEO. D. RAND.—This mineral, for which I propose the name *Ivigtite*, from its locality, was first observed in 1866, but only recently has been obtained in sufficient quantity for examination.

It occurs disseminated in films and seams through massive cryolite—sometimes forming a coating between crystals of carbonate of iron and the cryolite in which the carbonate is imbedded. Color, pale yellowish-green, sometimes yellow. Structure, fine granular, approaching micaceous. Hardness, 2—2·5. Sp. gr. 2·05. B. B. alone blackens slightly and fuses rather easily to a white slag. With carbonate of soda fuses readily and with effervescence to a greenish bead. In borax dissolves readily with an iron reaction. In microcosmic salt dissolves readily except silica skele-

ton; bead yellow while hot, bluish opalescent when cold. In closed tube yields acid water.

With considerable difficulty 0·679 gram of the mineral, free from admixture, was obtained and submitted to anlysis with the following percentage result:

Water,	3·42
Fluorine,	·75
Silica,	36·49
Sesquioxyd of iron,	7·54
Alumina,	24·09
Soda,	16·03
Loss,	11·68
	100·00

The very small quantity of the mineral which could be procured prevented a more satisfactory result, but from the foregoing characteristics I feel justified in pronouncing it a new species, and hope that a larger quantity may be procured and a correct analysis made.

Pachnolite, besides its usual occurrence in honey-combed cryolite, nearly or always in juxtaposition with the so-called Hagemannite, has been observed in crystals implanted on massive cryolite, and also coating crystals of the latter, mixed with microscopic crystals of cryolite. The crystals of pachnolite are always small, rarely exceeding the fiftieth of an inch in diameter, but those of cryolite have been found measuring over $\frac{3}{10}$ths of an inch cube. The crystals of carbonate of iron, found in the cryolite, have probably never been excelled for beauty. They are usually simple rhombohedrons, often of fine polish, and measuring from half an inch to four inches across. A black blende, containing much iron, has been found in the massive cryolite, crystallized in perfect octahedrons.—*Proc. Acad. Sci. Philad.*, 1868, 142.

2. *Notes on the Later Extinct Floras of North America, with Descriptions of some New Species of Fossil Plants from the Cretaceous and Tertiary Strata;* by J. S. Newberry.*—The following are the observations in this paper preceding the descriptions of species:

It is only within the last ten years that we have obtained any information whatever in regard to the nature of the vegetation which clothed the land that represented North America during the Cretaceous period. Previous to that time large collections of fossils had been made from rocks of that age on the Atlantic and Gulf coasts, but the beds which furnished them were marine sediments, and the fossils they contained were principally molluscs and radiates, but included also fragments of skeletons of Cretaceous saurians, *Mosasaurus*, *Hadrosaurus*, etc., and teeth of *Ptychodus*, a Selachian fish. In these remains there was found a generic correspondence with those of the middle and upper Cretaceous beds

* Read April 22d, 1867. Annals of the Lyceum of Natuural History in New York, vol. ix, April, 1868.

of the Old World, and many species were recognized as the same found there. In 1855, Dr. F. V. Hayden made the second of his numerous journeys into the country bordering the upper Missouri, which have resulted in such important contributions to our knowledge of the geology of the interior of the continent. At this time he was connected as Geologist and Naturalist with an exploring party sent out by the War Department under command of Lieut. (now Gen.) G. K. Warren, Corps of Topographical Engineers, U. S. A.

In the great mass of interesting materials brought by Dr. Hayden were a number of angiospermous leaves, obtained from a red sandstone lying at the base of the Cretaceous formation at Blackbird Hill, in Nebraska. Outline sketches of some of these leaves were sent to the distinguished fossil Botanist, Prof. Oswald Heer, of Zurich, Switzerland. By him they were pronounced of Miocene age, and referred to the genera *Laurus*, *Populus*, *Liriodendron*, etc.; a narrow lanceolate leaf being considered identical with *Laurus primigenia* Ung.; a broad rounded one with *Populus Leuce* Ung., both found in the Miocene of Europe. At the same time the fossils themselves were submitted to me for examination, and regarding the so-called *Populus Leuce* as generically identical with some large rounded leaves described by Zenker from the Cretaceous sandstone of Blankenburg, Germany, I considered this florula as of Cretaceous age—confirming the conclusions of Messrs. Meek and Hayden, who on other evidence had referred the deposit from which they came to that period. The plant called *Laurus primigenia* by Prof. Heer I considered a *Salix*, and the other leaves as representing the genera *Platanus*, *Populus*, *Fagus*, *Liriodendron*, *Sassafras*, *Magnolia*, etc. Unfortunately, Prof. Heer had only sketches, and those of but part of these leaves; and while I had the specimens all before me, I had no specimens of the Cretaceous flora of Europe, but only figures and descriptions of the comparatively few leaves up to that time found in this formation by Zenker, Dr. Debey, Steihler and others. It was, therefore, quite impossible that we could then make an intelligent comparison of the two floras. The genera recognized among these plants by Prof. Heer and myself were, for the most part, living in our forests, and largely represented in the Miocene strata of Europe. It is not surprising, therefore, that Prof. Heer should have considered them of Tertiary age, and that this opinion should be shared by many others.

Soon after the discovery of these plants by Dr. Hayden, he went again to Nebraska and Kansas, accompanied by Mr. Meek, and collected from various exposures of lower Cretaceous sandstone numerous additional specimens of the same and different species. Subsequently, I went myself to the region where these were collected, and spent some years in the study of the geology of the interior of the continent, exploring a large area occupied by Cretaceous rocks in Kansas, Colorado, Arizona, New Mexico and Utah. During these explorations I obtained from the Cretaceous strata,

at a great number of localities, angiospermous leaves, consisting of some of the species obtained by Dr. Hayden, with many others, all of which are described in the report of the San Juan expedition not yet published. In numerous instances, as Dr. Hayden had done, I obtained these leaves from the sandstones overlaid by calcareous beds containing *Gryphæa Pitcheri*, *Inoceramus problematicus*, and many other unmistakable Cretaceous fossils. These leaves I found to be characteristic of the strata in which they were first discovered, and was able to obtain them at nearly every exposure which I examined. In the end, I had before me, collected by Dr. Hayden and myself, at least fifty distinct species of leaves of this character from this horizon, with fragments, scarcely sufficient for description, of perhaps as many more.

Though Mr. Meek, Dr. Hayden and myself had thus demonstrated the true position first taken by us in regard to the age of the beds which furnish these leaves, the flora they represented was so modern in its character that the European paleontologists were still unwilling to admit the possibility of its being older than Tertiary; and it was only when, in 1863, M. Marcou and Prof. Capellini made a special journey to Nebraska, and collected fossils from the same localities that had yielded them to Meek and Hayden, that the fact was admitted that this flora was really of Cretaceous age.

The plants collected by Messrs. Marcou and Capellini embraced sixteen species, which have been described by Prof. Heer in the "Memoires de la Societé Helvetique des Sciences Naturelles, 1866," viz: "*Populus litigiosa*, *P. Debeyana*, *Salix nervillosa*, *Betulites denticulata*, *Ficus primordialis*, *Platanus* (?) *Newberryana*, *Proteoides grevilliæformis*, *P. acuta*, *P. daphnogenoides*, *Aristolochites dentata*, *Andromeda Parlatorii*, *Diospyros primæva*, *Cissites insignis*, *Magnolia alternans*, *M. Capellinii* and *Liriodendron Meekii*.

It is an interesting fact that of these sixteen species but three are identical with those obtained before from the same quarries, or those collected by myself elsewhere at the same geological horizon—an illustration of the richness of the flora which they represent. My own observations prove this richness still more clearly, for, as I have said, in the outcrops of the lower Cretaceous rocks of the West I have detected at least a hundred species of conifers and angiospermous trees. Of these it rarely happened, that in the chance exposure of a cliff or water-washed surface, anything like a perfect specimen could be detached and brought away. As a consequence we have, in the figures and descriptions now published or prepared, but a very imperfect view of the flora of the Cretaceous period on this continent, even as it has been exhibited to my eyes; and there is every reason to believe that but a small proportion of its elements have as yet been observed at all.

On the western margin of the continent it is well known that the Cretaceous strata are quite largely developed, having been recognized in Sonora, California, Oregon, Washington Territory

and Vancouver's Island. From the latter locality quite a number of fossil plants have been collected, which have been described by Prof. Heer, Mr. Lesquereux or myself. The first knowledge which we obtained of the Cretaceous beds of Vancouver's Island was derived from the descriptions by Mr. Meek (Transactions of the Albany Institute, vol. iv, p. 37) of some fossil molluscs collected by Dr. Turner. Subsequently, in 1858, the collections made by the United States Northwest Boundary Commission were placed in my hands for examination. These included fossil plants from the coal beds of Nanaimo, Vancouver's Island, which were associated with *Inoceramus*, *Pholadomya*, etc., before described by Mr. Meek, and which plainly indicated their Cretaceous age. These plants were described by the writer in 1863 (Boston Journal of Natural History, vol. vii, No. 4). Previous to that time the fossil plants collected by Dr. Evans, United States Geologist for the Territory of Oregon, were committed to Mr. L. Lesquereux, the well-known Botanist, who published descriptions of them in the American Journal of Science. Of these the following were from Nanaimo, viz:

Populus rhomboidea Lesqx., *Quercus Benzoin* Lesqx., *Quercus multinervis* Lesqx., *Quercus platinervis* Lesqx., *Salix Islandicus* Lesqx., *Cinnamomum Heerii* Lesqx., *Ficus sp.* Lesqx., with which are enumerated, but not described in full, "a *Platanus* with the same nervation as *Quercus platinervis*," a *Chamærops* agreeing with *Sabal Lamanonis* Bergh., common in the European Miocene, a very fine *Salisburia*, very variable in the outline of its leaves, and named *Salisburia polymorpha*, also a small piece of a fern referable to the genus *Lastrea*, and a *Sequoia* probably identical with *S. sempervirens*.

The Bellingham Bay plants described by Mr. Lesquereux consisted of species of *Smilax*, *Quercus*, *Planera*, *Cinnamomum*, *Persoonia*, *Diospyros* and *Acer*. By Mr. Lesquereux the plant-bearing strata of Bellingham Bay and Vancouver's Island were regarded as of the same age, and from the resemblance of the species they contain to those found in the Miocene of Europe, he pronounced them to be of that date (op. cit., vol. xxvii, p. 362). In a subsequent number of the American Journal of Science (vol. xxviii, p. 85) is published a letter from Prof. Heer upon these plants, of which sketches had been sent him by Mr. Lesquereux. In these notes the extinct flora of Vancouver's Island and Bellingham Bay are considered of the same age, and brought still nearer the Miocene of Europe; quite a number of species being regarded as identical with those found at Oeningen, etc.

Since that time a collection of fossil plants made by Dr. C. B. Wood at Nanaimo, V. I., and at Buzzard's Inlet, British Columbia, was sent by Dr. Hooker to Prof. Heer for examination. From the coal mine at Nanaimo but a single species of this collection was obtained—a conifer, considered by Prof. Heer as identical with *Sequoia Langsdorfii* Br. sp., a species common in the Miocene of Europe. From these facts it will be seen that the modern aspect

of the fossil flora of Vancouver's Island has produced the same misapprehensions as the Cretaceous flora of Nebraska. This, however, is not to be wondered at, and conveys no reproach to the eminent scientific men who have been misled by it. The identification of species by few and fragmentary specimens, or still worse by sketches, is a difficult and hazardous task for any one to perform; and in regard to the generic relations of the plants described, it can only be said, that previous to the discovery of such modern genera as *Liriodendron*, *Magnolia*, *Sassafras*, etc., in the Cretaceous rocks, they were naturally regarded as belonging to the present or Tertiary flora. It is also true that the flora of the Cretaceous period in the Old World has until recently been considered, from the number of Cycads it includes, as a continuation of the Jurassic flora; and it contains East Indian forms, none of which have as yet been discovered on this continent. There is no more doubt, however, that the plant-bearing strata of Vancouver's Island are Cretaceous than in regard to those of Nebraska. A very large number of Cretaceous molluscs have been collected, both in the overlying beds and those containing the plants, as was stated by the writer in 1863, in the report on the fossils collected by the Boundary Commission.

As regards the strata containing the plants and coal of Bellingham Bay, further observations and collections must be made there before the question can be said to be definitely settled. Mr. Gabb, paleontologist to the California geological survey, who has recently visited Bellingham Bay, has been led by the molluscous fossils obtained there to consider all the coal-bearing series of that district as Cretaceous. If this be so, there has been some error in the labeling of specimens which have come into my hands professedly from "Bellingham Bay." Some of them are unquestionably Miocene, for they include *Glyptostrobus Europœus*, *Taxodium occidentale*, and other plants found in the Miocene strata of Dacotah and Montana. The truth probably is that both formations are represented at or near Bellingham Bay. The coal of Goose Bay and the fossiliferous strata at Astoria are known to be Miocene, as are also the plant-bearing beds at Birch Bay and Buzzard's Inlet, and I have lately received a beautiful collection of Miocene plants from a locality not far distant in the interior.

From Orcas Island, which occupies an intermediate position between Bellingham Bay and Vancouver's Island, a collection of plants was made by Mr. George Gibbs of the Boundary Commission, in which the species are, with perhaps one exception, different from those obtained from the other two localities mentioned. These include ferns, palms and broad-leaved plants described in the report to which I have alluded, where they are referred to the Cretaceous period.

Combining the contributions thus made to our knowledge of the Cretaceous flora, and referring to this formation all that we now know to belong there, we have the following list of genera and species:

N. A. Cretaceous Plants now or hitherto described.

Populus rhomboidea *Lesqx.* Nanaimo.
Salix Islandica *Lesqx.*, "
Quercus Benzoin *Lesqx*, "
Quercus multinervis *Lesqx.*, "
Quercus platinervis *Lesqx.*, "
Cinnamomum Heerii *Lesqx.*, "
Salisburia polymorpha *Lesqx.*, "
Aspidium Kennerlii *Newb.*, "
Sabal sp., *Newb.*, "
Taxodium cuneatum *Newb.*, "
Ficus (?) cuneatus *Newb.*, Orcas Is.
Tæniopteris Gibbsii *Newb.*, "
Sphenopteris (Asplenium) elongata *Newb.*, Orcas Is.
Populus Debeyana *Heer*, Nebraska.
P. litigiosa *Heer*, "
Salix nervillosa *Heer*, "
Platanus Newberryana *Heer*, "
Andromeda Parlatorii *Heer*, "
Diospyros primæva *Heer*, "
Phyllites Vannonæ *Heer*, "
Aristolochites dentata *Heer*, "
Cissites insignis *Heer*, "
Ficus primordialis *Heer*, "
Magnolia alternans *Heer*, "
M. Capellinii *Heer*, "
Liriodendron Meekii *Heer*, "
Betulites denticulata *Heer*, "
Proteoides daphnogenoides *Heer*, Nebr.
P. acuta *Heer*, Nebraska.
P. grevilliæformis *Heer*, "
Leguminosites Marcouanus *Heer*, Nebr.
Sapotacites Haydenii *Heer*, Nebraska.
Populus cyclophylla *Heer*, "
Phyllites obcordatus *Heer*, "
Sassafras cretaceum *Newb.*, "
Liriodendron primævum *Newb.*, "
Araucaria spatulata *Newb.*, "
Quercus salicifolia *Newb.*, "
Magnolia rotundifolia *Newb.*, "
Platanus latiloba *Newb.*, "
Fagus cretacea *Newb.*, "
Sphenopteris corrugata *Newb.*, "
Pyrus (?) cretacea *Newb.*, "
Populus elliptica *Newb.*, "
P. microphylla *Newb.*, "
P. cordifolia *Newb.*, "
Acerites pristinus *Newb.*, "
Alnites grandifolia *Newb.*, "
Salix flexuosa *Newb.*, "
S. cuneata *Newb.*, "
S. membranacea *Newb.*, "
Quercus antiqua *Newb.*, S. Utah.
Quercus sinuata *Newb.*, "
Cupressites Cookii *Newb.*, New Jersey.

From this list it will be seen that the Cretaceous strata of the west coast include some forms not yet discovered in the Kansas and Nebraska beds. Among these, *Salisburia*, *Sabal*, *Cinnamomum*, etc., are indicative of a warmer climate. Possibly these genera may hereafter be detected in the plant-beds of Kansas, Nebraska and New Mexico, but as yet we have no intimation of their existence, and there is nothing now known in the Cretaceous flora of that region which gives it a tropical or even sub-tropical character.

It will be remembered that this vegetation grew upon a broad continental surface, of which the central portion was considerably elevated. This would give us physical conditions not unlike those of the continent at the present day; and it would seem to be inevitable that the isothermal lines should be curved over the surface somewhat as at present. It may very well happen, therefore, that we shall find the palms and cinnamons restricted to the western margin of the Cretaceous continent. It will be seen by the notes now given of the Tertiary flora of our continent, that, at a later date, palms grew in the same region where these Cretaceous plants are found; but cinnamons and other tropical plants seem to be entirely wanting in the Tertiary flora of the central part of the continent, while on the west coast both palms and cinnamons lived during the Tertiary period as far north as the British line. We have therefore negative evidence from the facts, though it may be reversed at an early day by further observations, that the climate

of the interior of our continent during the Tertiary age was somewhat warmer than at the beginning of the Cretaceous period, and that during both, the same relative differences of climate prevailed between the central and western portions that exist at the present day.

3. *On Human remains along with those of the Mastodon in the drift of California;* by Dr. C. F. WINSLOW.—Dr. Winslow, in a letter dated Baden Baden, Sept. 24, 1868, has called our attention to the following paragraphs in the Proceedings of the Boston Society of Natural History for 1857, p. 278, taken from a letter of his presented to the Society at the meeting of October 7, 1857 (11 years since), along with a fragment of the human cranium referred to. Dr. Winslow writes: "I sent by a friend, who was going to Boston this morning, a precious relic of the human race of earlier times, found recently in California, 180 feet below the surface of Table Mountain. As it is the first organic sign of human existence preceding or coëval with a drift age, or a general or minor 'deluge,' that has been found in the earth, I have thought it would be interesting for the scientific gentlemen of Boston to discuss the subject, and for a portion of the fragment sent to me to be preserved in the cabinet of your Society of Natural History. My friend Col. Hubbs, whose gold-claims in the mountains seem to have given him much knowledge of this singular locality, writes that the fragment was brought up in 'pay dirt' (the miners' name for the placer gold drift) of the Columbia claim, and that the various strata passed through in sinking the shaft consist of volcanic formations entirely. Whether his knowledge is accurate touching the volcanic formations I have some doubt, and have written for more certain information.

"The mastodon's bones being found in the same deposits, points very clearly to the probability of the appearance of the human race, on the western portions of North America at least, before the extinction of those huge creatures. As I have fragments of *Mastodon* and *Elephas primigenius*, or a kindred species, taken between ten and twenty feet below the surface, among the upper placer gold deposits of the same vicinity, it would seem that man was probably contemporary, for a certain period, with the closing dynasties of these two formidable races of quadrupeds. This discovery of human and mastodon remains in the same locality gives also great strength to the possible truth of the old Indian tradition of the contemporary existence of the mammoth and aboriginals of this region of the globe."

Dr. Winslow states in his letter from Baden Baden that this discovery was made in 1855. He adds also: "I have a fragment of a gigantic bone which Leidy examined and wrote me that it belonged to an extinct Sloth. It came from beneath the most populous part of the city, 30 feet, perhaps, below the surface. Another smaller, from another locality outside the city west, which he also pronounced as a Sloth, extinct and undescribed. I took fragments of trees and serpentine stones bored by *Lithodomi*, lying

pell-mell together from 60 to 90 feet below the surface in a tunnel not far from the western shore of the peninsula."

4. *Meek on the shell structure of some Spirifera.*—Mr. Meek's paper on this subject, referred to on p. 262, was read before the Academy of Natural Sciences of Philadelphia in December, 1865, and published in the Proceedings of the Academy, February, 1866.

III. BOTANY AND ZOOLOGY.

1. *Botanical Notices.*—Two new parts (containing three fasciculi) of the *Flora Brasiliensis* of von Martius have been issued during the past summer. The most considerable is fasc. 44, containing the *Loranthaceæ*, the work of the indefatigable Eichler, 134 pages of letter press and 44 plates, all the analyses by Eichler himself. The Brazilian *Loranthеæ* are all referred to *Psittacanthus* of Martius, a genus of undoubted character, and four others, two of them long ago proposed by Martius, which are conceded to be less distinct from *Loranthus.* The *Visceæ* of Brazil comprise 33 species of *Phoradendron*, an *Ixidium*, this being a genus founded by Eichler on one Brazilian species and the Cuban *Eremolepis Wrightii* of Grisebach, and an *Eubrachion.* In a note we have a synopsis of Eichler's West Indian genus *Dendrophthora* (*Arcenthobia* of Grisebach) 6 species. Fasc. 45 contains the *Loganiaceæ* by Dr. Progel, a new collaborator; seven genera, of which only *Spigelia* and *Strychnos* are rich in species; 16 plates; also the *Oleaceæ* and *Jasmineæ* by Eichler, with three plates. The *Chionantheæ* are still said to have "semina exalbuminosa," which, as we have long ago indicated, is not the case in the original *Chionanthus.* In fasc. 46, Dr. Seubert describes the *Styracaceæ*, two species of *Pamphilia* and 22 of *Styrax*, illustrated by five plates.

DeCandolle's Prodromus, vol. xvi, section 2, fasc. 2, issued in June last (500 pages), contains the *Betulaceæ* by Regel; the *Salicineæ* by Anderson, except the Poplars which are by Wesmael; the *Casuarineæ* by Miquel; the *Gnetaceæ* and the *Coniferæ* by Parlatore; and the *Cycadaceæ* by A. DeCandolle himself; to which are added the *Resedaceæ*, by T. Müller; and the *Lacistemeæ*, *Gunnereæ*, *Ancistrocladeæ*, *Dipterocarpeæ*, *Lophiraceæ*, *Monimiaceæ*, *Helwingiaceæ*, &c., by the editor. This important volume, especially as to the *Coniferæ*, opens a wide range for commentary, upon which we may not here enter.

No. 2 of the 12th volume of the 7th series of the Memoirs of the Imperial Academy of Sciences of St. Petersburgh (1868) is occupied by Fr. Schmidt's *Riesen in Amur-lande und auf der Insel Sachalin*, the botanical part, with two maps and eight plates. The plants collected in Amur-land and on the way to Sachalin (nearly 500 species) are first enumerated; and this is followed by the still more interesting *Flora Sachalinensis.* Noting the points most remarkable as respects the relations of this flora with that of the United States, we observe that the Japan *Trautvetteria* is kept distinct from the North American; *Diphylleia Grayi* from *D. cymosa;*

Caulophyllum robustum from *C. thalictroides; Vitis Thunbergii* from *V. Labrusca* (for which a good diagnosis, not yet published, has recently been made out by Prof. Braun); *Pyrus sambucifolia* is said to have larger fruit than *P. Aucuparia*, which also occurs in the islands; *Phellopterus littoralis* is adopted in place of *Glehnia littoralis* for the plant called *Cymopterus? littoralis;* a new *Stenanthium* is described and figured, &c. *Ehrharta caudata* of Monro, rediscovered in Sachalin, becomes the type of a new genus *Brylkinia.* A. G.

2. *Contributions to the Fauna of the Gulf Stream at great depths;* by L. F. de Pourtales, Assist. U. S. Coast Survey, (from the Bulletin of the Mus. Comp. Zoöl., Cambridge).—The author introduces his paper, describing the species observed by him, with the following remarks.

The study of the constitution and of the inhabitants of the bottom of the sea is a field of research which has attracted the attention of naturalists in comparatively recent times. What Humboldt did with regard to the distribution of life at different heights in the atmosphere, was done by Edward Forbes for the different depths of the ocean. The former's diagrams of the zones of vegetation on the slopes of the Andes are considered indispensable in every atlas of physical geography. But what one man could do where his glance embraced miles of country in height and breadth and where the type of vegetation could frequently be recognized as far as the eye could reach, an investigator even as zealous as Forbes could but sketch in broad though happily drawn lines for the marine animals.

Much has been done in this direction since Forbes's death, particularly in England, where dredging has become a favorite occupation of many naturalists; the Scandinavian seas have also been explored with much success, chiefly by the Norwegian naturalists; but much more remains to be done in a field in which the areas to be explored can, as Jeffreys remarks, be reckoned in square degrees, whilst the research extends only over several square yards.

It is particularly in the greater depths, in the so-called abyssal region, that our knowledge is deficient. This is easily understood, since on many coasts the sea is comparatively shoal for a considerable distance from land, and the outfit for deep sea dredging is beyond the means of but few private individuals. Government expeditions are generally fitted out for other duties, and can rarely devote their time to operations occasioning a delay of many hours. Furthermore, owing to the scantiness of the material, the impression generally prevailed, until recently, that animal life was soon reduced to a minimum with an increase of depth, or at least reduced to the lowest forms, so that the incentive of a rich harvest seemed denied to those who would have undertaken such researches.

Excepting the investigations of Dr. Stimpson on the coast of New England, the dredge has been as yet very little used along our shores. The character and constituents of the bottom are

however pretty well known, thanks to the care of the late Superintendent of the Coast Survey, Professor A. D. Bache, who, during his whole administration of that work, required the hydrographical parties to preserve the specimens brought up by the lead. From eight to nine thousand specimens have thus been accumulated at the coast survey office, from a region comprised between the shore and the outer edge of the Gulf Stream, and reaching nearly to 1500 fathoms. But, of course, aside from the Foraminifera and Diatomaceæ, for the study of which this material has proved of high interest, not much was contributed to our knowledge of the animals of the higher classes, the instrument used being only adapted to procure a small quantity of sand or mud.

The present Superintendent of the Coast Survey, Professor B. Peirce, has lately directed the resumption of the investigations of the Gulf Stream, so successfully inaugurated by his predecessor, but interrupted for several years by the war. Besides observations of the depth, velocity, and direction of that current, and the temperature and density of the water at different depths, the researches will be extended to the Fauna of the bottom, of the surface, and of the intervening depths. Not only will an insight be thus obtained into a world scarcely known heretofore, but that knowledge will have a direct bearing on many of the phenomena of that great current. Thus a new light may be thrown on its powers of transportation from shallow to deeper water, or along its bed, on its action in forming deposits in particular localities, or on its possible influence on the growth of coral reefs on its shores.

The first campaign on this plan was organized in 1867, the field of research being in a section between Key West and Havana, incidentally with the purpose of sounding out the line for the telegraph cable, shortly afterwards laid between these two points. The Coast Survey steamer Corwin was assigned to the work; and here I wish to express my thanks to my colleague, Assistant H. Mitchell, charged with the physical part of the campaign, and to Captain Platt and his officers for the interest they showed in my work, and for their valuable practical aid.

The expedition was unfortunately interrupted by the breaking out of yellow fever on board, so that the dredgings were few in number. However, short as the season's work was, and few as were the casts of the dredge, the highly interesting fact was disclosed, that *animal life exists at great depths, in as great diversity and as great an abundance as in shallow water.*

The identifications of the species has been made by me at the Museum of Comparative Zoölogy at Cambridge, in the rich collections of which I have found abundant material for comparison; facilities of every sort were afforded me by Professor Agassiz, for which I wish to express my heartfelt thanks, as also for this opportunity of prompt publication.

The first dredgings were made on May 17th, on the Florida side

of the Gulf Stream, about 5 miles S.S.W. of Sand Key, in depths varying from 90 to 100 fathoms, on a bottom of calcareous mud. The following list comprises the animals obtained:—

Articulates.—A number of small Crustacea were brought up, which have not yet been determined. They belong to the following or allied genera; *Dromia*, *Ilia*, *Mithrax?* (a mutilated specimen), *Pagurus*, *Euphausia*, and *Orchestia*.

The tubes of several species of Annelids were obtained, but the animals were in most cases too defective for identification. The largest and best preserved is *Morphysa floridana*, nov. sp. (see description). There are also tubes of one or more species of *Serpula*.

The Gephyreans are represented by *Sipunculus corallicola*, Pourt. (Proc. Am. Assoc., 1851.)

Molluscs not determined specifically. These are mostly immature specimens or fragments of dead shells, and belong to the following genera: *Murex* (dead), *Turbo?* (operculum), *Leda* (living), *Astarte* (living), *Tellina* (dead). Of Pteropods dead shells of the following species: *Hyalea tridentata*, *Hyalea trispinosa*, *Cuvieria columella*, *Cleodora lanceolata*. The shells of this order are very common in deep-sea soundings. The Bryozoa are represented by *Vincularia margaritacea*, nov. sp. (see description).

Radiata.—Of Echinoderms were obtained an *Ophiurian* (an arm, undetermined) and a number of specimens of *Comatula Hagenii*, nov. sp. (see description).

A *Zoanthus*, rather small, was obtained also, but not having been noticed when alive, it would be somewhat uncertain to determine.

Hydroids: *Antennularia triseriata*, nov. sp.; *Thoa pulchella*, nov. sp.; *Th. capillaris*, nov. sp. (see descriptions).

The Foraminifera had nearly all been washed out of the dredge; only the following were noticed: *Textilaria conica* D'O. (very large); *Operculina* (*Spirillina*) *incerta* D'O.; *Rotalina cultrata* D'O.; and *Globigerina rubra* D'O.

The total for this locality is therefore twenty-nine species, to which a few ought to be added for the undetermined fragments of Annelids.

No dredgings were had in mid-channel; this part has been reserved for the return trip, but the unfortunate interruption of the cruise prevented the execution of the project, at least for this season.

The next casts were obtained off Havana in 270 fathoms on May 24th and 29th, on both days as nearly as possible on the same spot, as the little that was obtained at the first date had given much promise.

The results of the two casts are combined below:—

Articulates.—The Crustacea are not determined, but are of or near the following genera: *Stenopus*, *Axia*, *Callianassa*, *Orchestia*, and *Idotea*, all living. Annelids: *Marphysa tibiana*, n. sp., and *M. antipathum*, n. sp. (see description). Tubes and fragments of four or five other species.

Of the *Molluscs* the Gasteropods and Acephala have not yet been determined, with one exception.

The following genera are represented: *Mitra?*, *Fusus*, *Turbo*, *Emarginulina*, *Dentalium*, *Nucula*, and *Spondylus*, all dead; *Pedicularia decussata*, Gould (see remarks), and a very small *Anomia*, both living. The Pteropods and Heteropods were all dead; they are: *Hyalea trispinosa*, *affinis* D'Orb., *gibbosa* Rang, and *uncinata* Rang; *Creseis spinifera* Rang; *Cleodora pyramidata* Pér. and Les.; *Spirialis rostrata* Eyd. and Soul.; and *Atlanta Peronii* Les. Of Brachiopods we obtained *Terebratula Cubensis*, n. sp., and *Terebratulina Cailleti* Crosse; both living and apparently abundant. The Bryozoa are: *Farcimia cereus*, n. sp.; *Vincularia margaritacea*, n. sp.; *Cellepora reticulata*, n. sp.; *C. sigillata*, n. sp.; *Canda retiformis*, n. sp.; *Canda cornigera*, n. sp., *Idmonea flexuosa*, n. sp. (see descriptions).

Radiata.—Echinoderms are represented by the following species; *Spatangus* (dead, fragments); *Fibularia* (dead); *Cidaris annulosa* Gray (probably, young, living); *Tripneustes ventricosus* (living, very young); *Asterias*, sp. (very young, living); *Ophiurians*, at least three species immature and difficult to determine; *Comatula brevipinna* n. sp., living; *Pentacrinus*, sp. (fragments of stem, among which some appear quite fresh).

Of Zoantharia the following were brought up: *Antipathes humilis*, n. sp.; *Antipathes filix*, n. sp.; *Acanthogorgia aspera*, n. sp.; *Gorgonia exserta* Ellis; *Swiftia exserta* Duch. and Mich.; *Hyalonema* (spicules); *Caryophyllia formosa*, n. sp.; *Deltocyathus Agassizii*, n. sp.; *Stylaster complanatus*, n. sp.; *Errina glabra*, n. sp.; *Errina cochleata*, n. sp.; *Crypthelia Peircei*, n. sp.; *Distichopora sulcata*, n. sp.; *Heliopora? tubulata*, n. sp.: *Heliopora? carinata*, n. sp.; *Isis?* (base of-stem); *Sarcodyction rugosum*, n. sp.

Hydroids: *Thoa pulchella*, n. sp.; *Tubularia crinis*, n. sp. Foraminifera: *Lagena striata* Mont., rare; *Nodosaria pyrula* D'O., rare; *Dentalina communis* D'O., rare; *D.* (*agglutinans?*); *Lingulina carinata* D'O; *Textularia trochus* D'G., common, very large also abundant in shoaler water; *T. agglutinans* D'O., rare; *Nonionina scapha*, rare; *Nonionina umbilicatula* Montg., rare; *Cristellaria crepidula* F. and M., rather common; *Orbiculina adunca* D'O., rare and only in a worn state, its proper habitat is in the littoral zone; *Amphistegina gibbosa* D'O., rare and only young specimens; it is very common throughout the Gulf of Mexico in deep water; *Globigerina rubra* D'O., very abundant, also in the Orbulina form; *Gl. Dutertrei* D'O., common; *Pullenia obliquiloculata* P. and J., rather common; *Pullenia coarctata*, n. sp., rather common; *Sphæroidina dehiscens* P. and J., not common; *Rotalina cultrata* D'O., very common; *Rot. truncatulinoides* D'O., common; *Rot. Poeyi* D'O., rather common; *Rotalina*, two other species in single and imperfect specimens; *Biloculina*, sp.; *Triloculina Brongniartiana* D'O., rare; *Quinqueloculina bicostata* D'O., rare.

Many of the specimens of Foraminifera are filled up with a yel-

low mass, like the first stage of transformation into greensand, but the process seems to stop here.

Of sponges quite a number were obtained, at least a dozen species, which have not yet been determined. Some of the detached spicules are remarkable for their size; one, for instance, of the slender rectangulated sexradiate type of Bowerbank measuring more than half an inch.

The vegetable kingdom was represented in this dredging by a single specimen of a minute alga, *Centroceras clavulatum* Agardh., which Harvey says was found abundantly at low water mark at Key West. In its branchlets was entangled a chain of a species of *Biddulphia*. Other Diatoms are rather scarce and have not yet been determined. We therefore find here, also, a confirmation of the remark made in European seas, that vegetable life does not extend to depths as great as are reached by animals, and that, therefore, the greater number of deep-sea animals must be carnivorous.

The dredge contained also a number of nodules of a very porous limestone, similar in color and texture to the limestone forming the range of low hills along the shore of Cuba, but composed apparently of the remains of the same animals which were found living. Thus our Deltocyathus, Caryophyllia, the various Pteropods were recognized in the stone, and found also in various stages of fossilization. The interstices between the larger forms are generally filled up with Foraminifera.

On May 25th the dredge was sent down in 350 fathoms, outside of the locality occupied on the 24th and 29th. It brought up only a few dead corals: *Caryophyllia formosa*, *Deltocyathus Agassizii*, *Diplohelia profunda*, the latter in numerous specimens (see description). Also a fragment of the siliceous skeleton of a sponge, forming a regular net-work somewhat like that of Euplectella as figured by Bowerbank, but lacking the spines.

The soundings made during the cruise seem to indicate a kind of submarine terrace, on which the dredgings of the 24th and 29th were made. The cast of the 25th was probably made on the edge of it, and the dredge no doubt touched bottom only for a short time, after which the ship drifted off into water too deep for the line attached.

3. *Deep-Sea dredgings in the region of the Gulf Stream*; by L. F. Pourtales. (Communicated by the author, to one of the Editors, Sept. 30, 1868).—I sent you a few days ago a small pamphlet* containing some of the results of the deep-sea dredgings made by me in connection with the exploration of the Gulf Stream by the Coast Survey. If you think it worthy of notice in the Journal of Science, I have thought it would add to the interest to mention the much more complete results of this year's campaign, which were the subject of a brief communication I made to the late meeting of the National Academy at Northampton. As the specimens have not all been determined as yet, I can give here but a short outline.

* The article above noticed.

The dredgings were made outside of the Florida reef, at the same time as the deep-sea soundings, in lines extending from the reef to a depth of about 400 to 500 fathoms, so as to develop the figure of the bottom, its formation and fauna. Six such lines were sounded out and dredged over, in the space comprised between Sand Key and Coffin's Patches. All of them agree nearly in the following particulars; from the reef to about the hundred fathom line, four or five miles off, the bottom consists chiefly of broken shells and very few corals, and is rather barren of life. A second region extends from the neighborhood of the hundred fathom line to about 300 fathoms; the slope is very gradual, particularly between 100 and 200 fathoms; the bottom is rocky and is inhabited by quite a rich fauna. The breadth of this band varies from ten to twenty miles. The third region begins between 250 and 350 fathoms and is the great bed of Foraminifera so widely extended over the bottom of the ocean.

The second region is the most interesting, from the variety of animals inhabiting it. The bottom-rock, of which many pieces were brought up, is a limestone, still in progress of formation from the debris of the shells, corals, etc., growing and dying on its surface. In this fauna the vertebrates are only represented by a very few small fishes, and those not deeper than 100 fathoms. But all the branches of invertebrates are represented; I will mention the most characteristic. Of the Molluscs, the most common is *Terebratula Cubensis*, *mihi*, and a new species of *Waldheimia*, both of large size. Of the former, more than a thousand specimens, and several hundred of the latter, were collected. Gasteropods are rarer and mostly small, the largest being the *Voluta junonia*, which was obtained living several times, and dead frequently. Acephala are rather rare and small, but Bryozoa are abundant. Articulates (Crustacea and Annelids) are well represented. But the great richness of this region lies in the Radiata. Of Echinoderms, the most common is a *Cidaris* (nov. sp.), besides which there are several new species of Echinidæ and very interesting Asteridæ and Ophiuridæ. Holothuriæ are rather rare, except a new Psolus. Of corals, I have eighteen new species, belonging principally to the families of Turbinolidæ and Oculinidæ; the Eupsammidæ are also represented by two or three species, the Fungidæ (a true Fungia) and the Milleporidæ by one each. The Madreporidæ and Astræidœ are entirely absent. There are also two or three species of Antipathes, eight or nine of Gorgonidœ, several of Actinidæ, some of them very abundant, Hydroid polyps, sponges, and Foraminifera. As a general rule, every thing is of small size. There are no seaweeds. Some animal remains are found whose presence is accidental, such as sharks' teeth, bills of Cephalopods, shells of Pteropods, &c., which have evidently come from near the surface, and also a considerable number of bones of the manatee, most frequently pieces of ribs; for the occurrence of the latter I am not able to account, as the manatee does not inhabit the open sea, and there are no currents to bring the floating carcasses from its usual haunts in the shallow bays.

From the third region the dredge brought up fewer though no less interesting specimens, the chief of which is a new Crinoid, belonging to the genus *Bourgueticrinus* of D'Orbigny; it may even be the species named by him *B. Hotessieri*, which occurs fossil in a recent formation in Guadaloupe, but of which only small pieces of the stem are known. I obtained half a dozen specimens between 230 and 300 fathoms, unfortunately more or less injured by the dredge.

The deepest cast made was in 517 fathoms; it gave a very handsome Mopsea, a crab, an Ophiurian, and some annelids.

The difference of the deep-sea faunæ of the opposite coasts of Cuba and Florida is very marked, although the distance is so small; of all the corals for, instance, described by me from the coast of Cuba, only two or three, and those in fragments, were found off the Florida reef.

The descriptions of the new species, with plates, are in preparation and will be published by the kindness of Prof. Agassiz in the next number of the illustrated Catalogue of the Museum of Comparative Zoology of Cambridge.

I am glad, also, to be able to say that Prof. Peirce, Superintendent of the Coast Survey, has directed me to continue these researches during the coming winter.

4. *On the genus Lælaps;* by EDWARD D. COPE. (Communicated for this Journal.)—As some confusion respecting the name of this genus has arisen, it appears best to attempt to correct it at as early a day as possible. This confusion, as it appears to me, has been created by remarks contained in an essay by Dr. Joseph Leidy, published in the last number of the Proceedings of the Academy, entitled "Remarks on a jaw-fragment of Megalosaurus."

In the Transactions of the American Philosophical Society, xi, p. 143, Dr. Leidy describes a large carnivorous reptile allied to Megalosaurus, under the name of *Dinodon horridus*. He assigns to it, with some expression of doubt, teeth of two distinct forms, viz: some having a lenticular transverse section, with crenation on the two margins in part, and others having a lenticular section, truncate to a greater or less degree, in place of its angles, and therefore crenate on three edges in part.

If Dr. Leidy had left the matter undecided as to which of these he regarded as the type of the genus *Dinodon*, the almost universal practice of naturalists would refer the name to that form which should not be first thereafter discovered to be distinct and named.

I have been of the opinion that the two forms of teeth included by Dr. Leidy under the head of Dinodon really belong to distinct animals, and Leidy is also of that opinion. In 1866, in describing the genus *Lælaps* (Proc. Acad., p. 279), I said: "The genus *Lælaps* belongs to the family *Dinodontidæ*, which is characterized * * by its compressed sabre-shaped teeth. It differs * * from Dinodon in that teeth of the latter have two posterior serrate edges separated by a posterior plane." This, then, according to the usage of naturalists, establishes the name of Dinodon for the truncate teeth, and Lælaps for the two-edged.

Dr. Leidy, however, in an essay just published (Proc. Acad., 1868, p. 198), in expressing his belief in the distinctness of the two genera, states that "teeth of like shape" (i. e., like Megalosaurus), "referred by me to Dinodon, alone belong to this genus," and names the species represented by the truncate teeth, or the true *Dinodon horridus*, *Aublysodon mirandus*. He then goes on to say: "Future discovery may prove Lælaps and Dinodon identical," and on p. 199: * * "An enemy which may perhaps on nearer comparison of corresponding parts prove to be another species of the same genus, until now supposed to be different, under the names of Dinodon and Lælaps." It is thus sufficiently obvious that the proposition is to refer Lælaps as a synonym of of Dinodon. It appears to me, on the other hand, that is contrary to the rules of nomenclature, and the principles which lie at their root, and that the name *Aublysodon* is a synonym of Dinodon.

This is, however, on the supposition that Leidy had left the question open or uncertain, as to which of the two forms of teeth was characteristic of his genus *Dinodon*. I think, however, he has not left it undecided, and I am supported in this by the opinion of von Meyer.

The teeth of Lælaps, both from New Jersey and Nebraska, do not differ from those of Megalosaurus, while those of Dinodon do. It was not to be supposed that Dinodon was established on teeth of the former character, as the practice of describing species and genera, without a basis of distinctive characters, is an unusual and bad one, and ought not to be tolerated in natural science.* In describing Dinodon, Leidy says the Lælaps-like teeth resemble those of Megalosaurus, and in his recent article in the Proc. Academy (p. 198), that they are "identical in character with those of Megalosaurus."

He, however, specifies that the truncate teeth of Dinodon are really those that characterize it, in the following words: "As the entire dentition of Megalosaurus has not yet been ascertained, it may turn out to be the case that in other parts of the jaws than those known it possesses teeth like the ones *above described as peculiar*. Should, on future discovery, such a condition of things be proved to exist, *Dinodon* would then cease to be anything more than a second species of Megalosaurus." The truncate teeth are then the "peculiar" feature of Dinodon, and all that prevents the species from being referred to Megalosaurus.

Von Meyer has understood this language as I have, and has believed that the teeth now ascribed by Leidy to *Aublysodon*, are really characteristic of Dinodon. He says (Palæontographica, vii, p. 267) that while some of the teeth are identical with those of Megalosaurus, "the others indicate such peculiarity, that Leidy,

* Falconer says of it: "It assumes a difference on theoretical grounds, where the direct evidence, so far as it goes, indicates the contrary; and its general adoption would tend to arrest on the threshold the investigation of the means through which remote geographical forms, presenting common characters, may have started from a common origin.—(*Nat. Hist. Review*, 1863, p. 64.)

who has made the investigation, thought it necessary to characterize the animal as distinct from Megalosaurus, under the name of *Dinodon horridus.*"

It is therefore evident that the Lælaps-like teeth described under Dinodon are really those that require a new name, if any. I will not give them a name, however, since there is no evidence that they differ from either Megalosaurus or Lælaps, though of course the probability is, that they belong to a species of the latter genus.

Although *Aublysodon* would thus be a synonym of Dinodon, it is not an altogether useless name, since the latter was given years ago to a genus of serpents by Duméril and Bibron, and may therefore be suppressed. The name of the family *Dinodontidiæ* also, which I gave in compliment to Dr. Leidy, may also be disused.

In the same way, a genus of extinct reptiles was distinguished as *Tomodon* Leidy, a name long since given to a genus of American serpents. Not liking to substitute a generic name given by another, by one of my own, I have requested Dr. Leidy to give me one by which to replace it.

The synonymy of these genera will then be

LÆLAPS Cope.

Dinodon Leidy, 1868, not of 1857.

LÆLAPS AQUILUNGUIS Cope, Proc. Ac. Nat Sci. Phil., 1866.

LÆLAPS MACROPUS Cope, sp. nov.—This species differs from the former in the relative proportions of the phalanges and tibia; the toes were relatively larger than in the preceding. Some of these bones are figured by Leidy (Cretaceous Reptiles), and described under the head of animals related to Hadrosaurus. It will be described more fully by me in my forthcoming memoir on the extinct reptiles of North America.

AUBLYSODON Leidy.

Dinodon Leidy, 1857, Proc. A. N. Sci. Phil., 1866, p. 198, not Duméril and Bibron.

AUBLYSODON HORRIDUS, *Dinodon horridus* Leidy, Trans. Amer. Philos. Soc., xi, p. 143. *Aublysodon mirandus* Leidy, Proc. Acad. Nat. Sci. Phil., 1866, 198.

5. *Considerations drawn from the study of Mole Crickets;* by SAMUEL H. SCUDDER, (from the Proceedings of the Boston Society of Natural History, vol. xi, January 22, 1868.)—Mr. S. H. Scudder stated that he had recently been studying the mole crickets with a view to their classification, and found that they were naturally divisible into two groups. For one he retained the name of *Gryllotalpa*, under which all the species had formerly been grouped, and to the other applied that of *Scapteriscus*. These two groups were separated by the following characteristics.

In *Scapteriscus* the posterior margin of the sternum of the eighth

abdominal segment of the ♂ is produced into a stout prominent central tooth; in *Gryllotalpa* the margin is entire.

The mesosternal ridge of *Gryllotalpa* is prominent, and almost equally so throughout; that of *Scapteriscus* is never prominent on the anterior half of the segment, and is often limited to the posterior half, or is even obsolescent.

The fore trochanter of *Scapteriscus* is large; the free portion almost always equals the tibial dactyl in length, and is of about the same size at the tip as at the base; that of *Gryllotalpa* is proportionally small, seldom exceeding half the length of the tibial dactyls; the form is cultrate or lenticular.

Scapteriscus is furnished with only two fore-tibial dactyls, both of which are movable; *Gryllotalpa* has two movable dactyls besides a second pair which are immovable.

With but few exceptions, the hind femora of *Scapteriscus* more than equal the pronotum in length, while in *Gryllotalpa* they are always shorter than the pronotum.

In *Gryllotalpa* the length of all the hind tarsal joints taken together seldom exceeds half the width of the pronotum, while they equal its whole width in *Scapteriscus*.

The hind tarsal claws of *Scapteriscus* are clothed with short hairs nearly to the tip; those of *Gryllotalpa* have hairs only at the base.

The tegmina of *Scapteriscus*, with but few exceptions, cover, when at rest, two-thirds of the abdomen; in *Gryllotalpa* they seldom conceal more than one-half of the abdomen.

The nervures of the middle field of the tegmina in the females of *Gryllotalpa* are distant and rather irregular, somewhat resembling those of the males; in *Scapteriscus* they are approximate, regular and straight.

The anal cerci are longer than the pronotum in *Gryllotalpa;* shorter in *Scapteriscus.*

Finally, the ninth, and sometimes the eighth abdominal segments are furnished above, in *Gryllotalpa*, with two transverse lateral rows of long hairs directed inward, as if to keep the long folded wings in place; these are absent from *Scapteriscus*, where the wings are equally long and similarly folded.

Only one species of *Scapteriscus* has been found without the limits of South and Central America, and that—occurring in a single instance in Europe—must undoubtedly be considered an emigrant from the same warm regions; the members of the genus *Gryllotalpa*, on the contrary, are found throughout the whole world, not excluding Central and South America.

Comparing these two genera with their nearest allies, *Tridactylus*, *Cylindrodes*, etc., we find great and striking differences—differences which are extraordinary compared with those which divide *Scapteriscus* and *Gryllotalpa;* the comparatively simple fore tibia, and the abnormal appendages which supplant the hind tarsi in *Tridactylus*—the abbreviated legs fitting into cavities in the body, and the absence of articulated appendages at the extremity of the abdomen in *Cylindrodes*—these characteristics are far more

important than the sexual sculpture of the abdomen, the ultimate neuration of the tegmina, the length of the legs, the contour of the trochanters, or the digitation of the tibiæ, which separate *Scapteriscus* and *Gryllotalpa*.

The facts cited above present two features which bear upon the question of the origin of species.

First: these little mole crickets, so unique in their structure as to be widely separated from their nearest allies, are spread uniformly over the whole surface of the globe; but few species occur in any one place, and at least one is found in every temperate or hot region.

Now, if species originate or change from physical causes, or by "Natural Selection," why is it that under every physical condition and surrounded by every variety of antagonism possible in their habitat, this same unique structural form has sprung up all over the globe?

Again, how can such theories account for another feature—common, indeed, to all natural groups—that it is not one striking characteristic which separates *Scapteriscus* and *Gryllotalpa*, and which "Natural Selection" might have seized upon, with reference to some special benefit, but a combination of features which have no apparent dependence upon each other, correlated, but not necessarily connected? Why should "Natural Selection," altering for its own purpose the palm of the four-fingered mole cricket into that of the two-fingered species in South America; or, developing in South America, from some previous synthetic form of mole cricket, both the present four-fingered and two-fingered species, and in other parts of the world the four-fingered species only—destroying at the same time the primæval form all over the surface of the globe—at the same time, place rows of hairs on the hinder part of the abdomen of the tetradactylate group, and none on that of the didactylate? or make the veins of the tegmina of the ♀ of one group, distant and irregular, and those of the other straight and approximate? Why furnish the eighth abdominal segment of the ♂ of one with a projecting tooth, and deprive those of the others of such a prominence? Why give one long and the other short anal cerci, or clothe the hind tarsal nails of one with short hairs and leave the other naked? What have these features to do with the differences of structure we have mentioned in the palm-shaped fore leg, or in the length of the hind leg? These and similar difficulties, arising on every hand, seem to attend every derivative theory of the origin of species.

6. *Remarks on two new fossil Insects from the Carboniferous Formation in America;* by S. H. SCUDDER, (ib., Feb. 26.)—Mr. Scudder exhibited two fossil insects from the coal measures. One was found in the iron-stone nodules of Morris, Illinois, which have previously afforded remains of insects. The fragment represents the wing—apparently an upper one—of a neuropterous insect, which he called *Megathentomum pustulatum*. It is gigantic in size, very broad, with distant nervures, simple and slight divarications, and in the outer half of the wing, which alone is preserved, a cross

neuration, composed of most delicate and irregular veinlets. The wing is also furnished with a large number of larger and smaller discolored spots, the surfaces of the larger ones irregularly elevated.

The *vena mediastina* is simple and straight; the *vena scapularis* sends out two branches from its upper side, the first of which does not reach the border but loses itself in a congeries of minute veins, while the second, branching again quite near its origin, supports the tip of the wing; the *vena externo-media* occupies the middle third of the wing, and divides once near the base; each branch is straight and forks again, the upper one a little nearer the border than the second divarication of the *vena scapularis*, the lower still nearer to the margin; the *vena interno-media* divides several times, the uppermost branches forking again just inside of the border; the *vena analis* does not appear on the fragment.

There are six larger round or squarish spots; four of them form a bent row a little beyond the middle of the wing, the upper three spots being nearly straight and the lower one turned inward at a little more than a right angle; the uppermost spot occurs in the interspace between the *venæ scapularis* and *externo-media;* the others follow in succeeding interspaces. The two other large spots are found in the same interspaces with the upper two of the inner row, and are situated about half way between them and the border. The smaller spots appear to be less regularly distributed; they are usually round, but sometimes oval or elongated; there are three at equal distances from each other in the lower outer interspace formed by the branches of the *vena scapularis*, one occurs just within and above the inner of the three just mentioned, and one near the angle of the last divarication of the *vena scapularis*; there are two between the forks of the upper branch of the same, and, in the interspace between the branches one spot is found close to the margin; two larger and elongated spots occur in the same interspace with the lowest of the four large spots and three equidistant round ones in the next interspace below; in the succeeding interspace, probably about half way between the base and the outer border, there is an oval spot; finally two faint ones are situated upon and beneath each of the branches of the *vena externo-media* near the middle of the wing.

The wing was probably a little more than three inches in length; its greatest breadth measured by a line at right angles to the costal border is 1·8 inch; from the apex of the wing, where the upper branch of the *vena scapularis* touches it, to the lowest point of the lower outer angle 2·1 inches; from the center of the upper, inner large spot to the outer margin 1·05 inches; greatest breadth of an interspace, ·34. This insect, apparently allied to the *Coniopterygidæ* by the simplicity of its neuration, differs from that family, not only in the cross-veining, but in the mode of branching and the proportion of the wing allotted to each of the veins. Dr. Hagen has shown me in this wing some resemblances to the *Phryganidæ*, but I am inclined to believe it is distinct in its family characteristics from any known type of Neuroptera.

The second insect, for which the name of *Archegogryllus priscus* is proposed, was found by Dr. J. S. Newberry in the lowest coal beds at Tallmadge, Ohio. In consists of a broken leg of a cricket and a very small fragment of its wing—apparently a lower one. There are no determinate characters in the wing. The leg was broken into fragments from which a femur and tibia could be made out; they are quite remarkable, for while the femur is smooth, the tibia is furnished with several prominences of large size; in modern types, the prominences, if they occur at all, are found only on the femur; in this specimen there is a slight rounded prominence on the upper surface at the very base of the tibia and another just beyond the middle; opposite the latter, on the upper surface, is a deeply bifid elevation, its hollow corresponding to the elevation on the upper surface; the basal half of the under surface is occupied by a very broad prominence, abrupt at its edges, of nearly equal height throughout, but slightly depressed in the middle. Length of the femur, ·28 inches; breadth of the same, ·11 inches; length of the tibia, ·26 inches; breadth of the same, ·045 inches.

7. *Siredon a stage in the development of Amblystoma;* by B. SILLIMAN.—Prof. Marsh has given on pp. 364–374 an interesting account of the transformation of *Siredon* into *Amblystoma*, illustrated by excellent figures.

The very limited time at our command at Como station, did not allow any opportunity to study the habits and actual condition of this remarkable and ambiguous reptile in its native habitat. All the metamorphoses described so clearly by Prof. Marsh occurred, it will be observed, subsequently to the removal of the collection of specimens made for us by the kindness of Supt. Snyder, to lower and warmer altitudes. This has led to some discussion of the question whether these changes in the animal would have occurred had these creatures remained in Lake Como.

On the return of our party from the terminus of the Union Pacific Railroad to Cheyenne, 120 miles east of Como, I left the party for a detour to the Colorado mines. This gave me an opportunity to see, in the possession of a person who had a sort of menagerie at Cheyenne, a number of *Amblystoma* which had been developed in the establishment from *Siredon.* These were living in a large glass fish-globe, and the proprietor assured me he had obtained them some weeks previously from Lake Como, and that when they arrived they were all in the "fish state"—much larger than at present and provided with 'gills,' as he expressed it, about the head. In a short time they commenced to change, and in about three weeks all were completely developed into Salamanders. They were six or eight in number, and when I saw them, they very closely resembled the specimens now in New Haven, and described by Prof. Marsh, with a like diversity of color and markings. I was quite particular in my inquiries of the owner, and am persuaded his account was entirely truthful, and he assured me we should find our specimens soon changing in the same manner. As this transformation was then unknown to me, and seemed very im-

probable, I received the statements with a measure of incredulity. More than three weeks elapsed before I again heard of our New Haven colony of Siredons, and I was not a little interested to learn, on my return here, that nearly the whole number had already undergone the transformation into *Amblystoma*.

We know, therefore, in the case of two different colonies of Siredons from the same locality, placed under very unlike conditions of elevation above the sea, but otherwise similarly situated, that both have undergone the same transformations. We cannot therefore avoid the conclusion that *Siredon* is a stage in the development of *Amblystoma*.

New Haven, Nov. 2d, 1868.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Earthquake of Aug. 13th—16th*, 1868.—We condense from newspaper accounts written immediately after the event and before accurate reports from distant points could be received, such a statement as we are able to make of the chief facts of scientific interest connected with this extraordinary convulsion of the earth. In extent and violence, as well as in destructiveness to life and property, it appears to have been one of the most remarkable experienced in South America since its settlement by Europeans. The whole western coast from New Granada to Chili was shaken, and cities and towns in great numbers were suddenly laid in ruins, either by the violence of the shocks, or by the subsequent influx of the sea, or by both. The loss of life is but imperfectly known, the estimates ranging from 30 to 60 thousand and upwards; some statements making it over 40,000 in Ecuador alone, where it was proportionally greatest on account of the first shocks occurring in the dead of night, while in other regions they took place for the most part in the daytime.

The shocks commenced on the 13th of August, about 5 o'clock P. M., and were felt throughout the coast provinces of Peru and Bolivia, the center of disturbance being in southern Peru. In different places the shocks lasted from two to seven minutes. The sea immediately retired to great distances, and returned on the coast at the rate of ten miles an hour, and with a wave full fifty feet high, that covered many towns and swept away in its reflux every thing within its power, leaving ships high and dry upon the land. The places where the convulsion was felt most severely are Arequipa about forty miles from the coast, and Moquegua about twenty-five miles inland. Arequipa, a city of fifty thousand inhabitants, situated but 14 miles from the lofty volcano called "Misti," was almost totally destroyed. Though solidly built of stone, not a church is left standing, not a house is habitable, nor scarcely one stone left upon another. Some two hundred of the inhabitants perished, the rest having had time to escape.

At this place the earthquake commenced with an undulating movement, and as the shocks culminated no one could keep his feet; the houses rocked like ships in the trough of the sea, and

came crumbling down. Nineteen minor shocks took place the same night, and the earth continued to be disturbed for some days. Openings occurred in the plains around the city, and water made its appearance in various places. The neighborhoods of Tiabaya and Sanbandia and all the outlets of the city shared her fate. The neighboring volcano opened on the side toward the north, and threw forth earth and ashes; the water which the inhabitants used to drink turned black, and of an insupportable taste.

Another important city almost totally destroyed was Iquique, in southern Peru. A correspondent of the New York Times writes:—"A few minutes after 5 P. M., on the 13th inst., the inhabitants were terrified by loud subterranean noises, presaging the approach of an earthquake. The shock immediately followed. Every building was shaken to its foundation. The population rushed into the streets, and those who were not buried by the falling walls made their escape into the country with all speed. Had they remained in the city the entire population must have perished, as immediately on the passing of the first shock, the sea retired for some distance, and then returned with a great rush and a roaring sound more terrible than the loudest thunder. The wave was of dreadful height and force, and it swept irresistibly over the town, completing the ruin begun by the earthquake. Not a mercantile establishment has escaped, nor does a vestige remain of the most opulent and handsome section of the town. The massive buildings belonging to the nitrate merchants, mostly constructed of stone and lime, have entirely disappeared. Buildings constructed of timber were swept away at once, leaving nothing to mark the place where they stood.

"The inroad made by the sea extended to the Pantera, part of which was covered by the wave, and that section of the town known by the name of the Puntella has been entirely destroyed, leaving nothing but the debris of innumerable houses.

"It is supposed that over 200 persons were killed by the earthquake."

Repeated shocks were felt subsequently to the 13th, although of a milder nature.

The establishment of the Tarapaca Nitrate Company at Molle, eight miles south of Iquique, has entirely disappeared, and other works in the nitrate districts have suffered materially. The works of the Peruvian Mining Company of London are entirely destroyed, upwards of 400 tons of silver ore, heavy machinery, and large quantities of quicksilver having been swept away like so much chaff.

In like manner Arica in Peru, the chief port for the commerce of Bolivia, after being destroyed by the earthquake, was obliterated by the sea. Of five ships that lay at anchor in the bay, all but one were destroyed with the entire or greater portion of their crews. The single exception was the United States war-steamer *Wateree*, which was swept a quarter of a mile inland, and lost but one man.

Some of the officers of the Wateree have reported the following facts:—

At 32 minutes past five o'clock, that is, 17 minutes from the cessation of the initiary shock, which lasted ten minutes, the first wave was experienced. These waves for a short period followed each other with great rapidity and regularity. The experience of those on the Wateree was peculiar. The water retired from the shore and then, unlike the regular pulsation or roll of the sea, rose from beneath, placing the ship, as it were, poised on the apex of a cone-like hill. This wave fell as suddenly as it rose, the steamer meantime shaking like a leaf in the wind; and then commenced a series of tidal rollers, first moving in grand masses toward the shore, and next retreating until those on the ship could see the ground of the roadstead almost from their anchorage to the shore. The vessel was swung and thrown hither and thither like a cork, and finally, on the ninth movement of the waters—the earth trembling without cessation, while the sky overhead was exceedingly clear and not a breath of wind stirred—was driven from her moorings and carried, losing one anchor and chain and taking the others with her, over the shore line and across the railroad, the track of which was destroyed, some distance inland, and finally left on an elevation of twelve feet on an otherwise level plain immediately north of the city, where she now lies apparently but little damaged, and, if she were worth it, in such a position that she can be moved back to the sea. The Wateree, being flat-bottomed, met with little obstruction, while the storeship Fredonia, being deep-keeled, was dashed against the rocks, and on the morning following the disaster, there was not a plank of her left that was whole. There were shocks of greater or less violence at Arica up to Aug. 30.

The destruction at Arica was complete. All public and private buildings, even the fort and the trees were swept away. Of the inhabitants some 200 perished.

At Callao the damage caused by the shocks was comparatively slight; that by the subsequent fire and inundation was very great.

The Port of Chala, about half-way between Callao and Iquique, suffered great damage, the havoc continuing for about forty-five minutes. At the moment the steamship *Santiago* was about to anchor, after a shock which was felt very sensibly on board, the sea receded, parting the chain of the vessel, and of the company's hulk, at anchor in the roadstead, and then returned at a height of about fifty feet, covering the rocks about the anchorage and in the harbor, and sweeping up into the town for the distance of over 1,000 feet. The Custom-house, Steamship Agency, Mole, and everything within range was swept away by three successive seas preceded and followed by as many as twelve shocks of earthquake, each lasting from three seconds to two minutes in duration.

The town of Tambo is entirely washed away, and upward of 500 persons have perished. The towns of Tiabaga Vitor, Molliendo and Mejia, and all the villages within 150 miles are totally

destroyed. In the latter place all the materials for the building of the Arequipa railroad, which were deposited there, were washed away. Mejillones is completely destroyed.

At Tacna it is said as many as sixty-four distinct shocks of earthquake were felt, and they continued up to the last date (16th). The towns of Sama and Lecomba are nearly destroyed. The earth opened in many places and vomited forth hot water. The valley of Lluta is completely ruined.

Owing to the elevation of the port, the damage done to Islay by the sea was not very great, though the earthquake wave rose to the height of sixty feet. The French ship *Canton*, although left high and dry, succeeded in floating off with the return sea. The mole is nearly destroyed. In the town every stone or cement wall is either demolished entirely or badly shaken. Among other freaks of the earthquake an entire new quebrada has opened at Guerreros, with a running spring at the bottom.

At the Chinca Islands the shocks were so strong as to throw every one to the ground. For a while after the subsidence of the earthquake the sea remained perfectly quiet, but about 9:30 o'clock at night commenced retiring, and when about seventy yards distant raised itself in an immense wave, which rushing forward threw itself with irresistible weight against the mole. The solid structure was instantly torn away. The inhabitants gave themselves up for lost, supposing the whole island was about to be submerged. The ships were dashed about like cockle-shells, and suffered great damage from striking against each other. All the launches and small vessels are totally destroyed, the wharves and the mole are so injured that immense sums will have to be expended on them before they can be of service.

In Ecuador repeated shocks were felt between the 13th and the 16th, the most destructive occurring at the latter date. At Guayaquil the first shock lasted forty or fifty seconds, the direction being from east to west, and the movement regular and slow. The exact date is not stated.

At Quito the severest shock, according to a Government circular, occurred a little after one o'clock A. M., of the 16th. It was prolonged and caused much damage to the city, with considerable loss of life. The greatest disturbance was in the province of Imbabura. Ibarra, its capital, San Pablo, Atuntaqui, Imantad, Otovala, and many other places are entirely in ruins. The shock happened at midnight, and was so sudden and violent that there was no time for any one to escape. In Ibarra, Otovala and Cotacachi almost the entire population perished. Where Cotacachi was is now a lake.

At Talcahuano, in Chili, no shocks had been felt on the 14th, but, the night preceding, an extraordinary wave from the sea had swept in upon the town, causing immense damage. At 10 o'clock at night the sea had gone back over 270 yards. A little before 11 o'clock it rose with a swell some 10 or 12 feet above its former level, and dashed upon the beach with an awful roar.

From 12 at night until 2 in the morning the sea was in great agitation, retreating and rushing forward by turns, and at last subsiding within its ancient bounds.

In Tome the town was likewise inundated.

In Constitucion the vessels anchored in the port were swept up the river, some of them being left aground and others sustaining considerable damage. The city itself was but slightly injured.

Earthquake Wave at the Sandwich Islands.—The oceanic disturbance caused by this earthquake was not confined to the South American coast, but extended across the Pacific, even to Japan. It was observed at the Sandwich Islands on the 14th, and for several succeeding days, and at Yokohama, Japan, on the 15th.

At Hilo, Hawaii, the phenomenon was first noticed at midnight of the 13th. A correspondent of the Honolulu Commercial Advertiser, writing on the 15th, states, that from that hour there had been a constant rise and fall of the ocean up to date, the fall being from 6 to 8 feet on the average, and occurring once in an hour through the day. The bridge at Waiakea had been lifted bodily from its position, and carried up the Waiakea as far as the fish-pond walls. This was done by the rise of 11 o'clock A. M. of the 14th. Another correspondent at Hilo states that the influx and reflux of the sea, as observed by his informants, had been taking place since the hour of 2 o'clock A. M. of the 14th, rising and receding every few minutes; from four to six feet being the general rise higher than usual tide. This continued throughout the day and night, and up to 4 P. M., the date of writing, on the 15th. A principal feature of the phenomenon noticed was its long continuance and regularity, varying little from five minutes in rising or receding, when receding the whole distance, sometimes falling but about one half the distance that it fell at other times.

From Kawaihae Mr. Conway writes, under date of Aug. 15th, that he was awakened between 3 and 4 o'clock on the morning of the 14th, by a peculiar rumbling noise, apparently as of stones rolled along the beach; and on going out, he saw the rocks on the beach, for several hundred yards, entirely bare. After a few minutes the water suddenly rushed in again with the rapidity of a six knot stream, and reached full thirty inches higher than the usual highest tides. After flowing and ebbing slightly for some ten minutes, with considerable agitation, it again receded to from two to three feet below the usual tide-marks; and so it continued to the time of writing, at longer or shorter intervals.

At Kealakekua, the extreme rise and fall was reported to have been about 10 or 12 feet.

From the Island of Molokai, Captain Fountain writes, under date of the 16th, that at 10 o'clock A. M. on the 14th, he noticed that the tide was four feet higher than usual, and that it rose and fell twelve times during the next four hours. The next day it continued the same, but longer between the time of rising and falling. On the 16th, it was continuing, but with the intervals still longer.

At Kahului, on the windward side of Maui, the phenomenon was first observed at early daybreak on the 14th, and continued during the day, leaving the reef and some rocks in the harbor quite bare. It was judged that the extreme rise and fall was nearly twelve feet. It was highest about 7 and 11 o'clock A. M. At places on the lee side of Maui, the rise and fall appears to have been not so great as at Kahului.

At Honolulu, the tide was first observed rising higher than usual about 9 o'clock P. M. of the 13th. At midnight the sea was receding, with a noise as of persons wailing, caused by the water rushing over the reef. At 7 o'clock the sea began to flow out rapidly, until 15 minutes past, when it checked and rapidly returned. The extreme fall, from the highest point, was three feet and ten inches. At 20 minutes past 8 A. M., the water again receded, and continued falling for 15 minutes, when it turned and rose 28 inches in eight minutes. Since that hour it continued rising and falling rapidly every 20 minutes, until about 25 minutes of 3 P. M., when it reached the highest point, five feet and four inches above the lowest mark.

At Waimea, Kauai, the tide rose and fell some six feet between the highest and lowest points observed, between 10 and 11 o'clock on the forenoon of the 14th. No measurements were taken.

It appears, from these reports, that at the Sandwich Islands the tidal oscillations were nearly uniform, throughout the group—that the tide rose highest and fell lowest on the windward or eastern shores of Maui and Hawaii—that it commenced nearly simultaneously at midnight, or a little before midnight, at the various points—that the greatest variations occurred at or about 7 and 11 o'clock A. M. on the 14th—that the oscillations continued from forty-eight to sixty hours from the commencement—and that at all the places where observed, the sea rose and fell *gradually*, though in some places more rapidly than at others, varying from four to twelve inches per minute.

The earthquakes of the same date in South America were obviously the cause of the phenomena observed at the Sandwich Islands and in Japan, the earthquake wave having occupied but a few hours in crossing the entire Pacific.

Earthquakes and earthquake waves in Australia and New Zealand.—On the morning of August 17 (Aug. 16 as compared with time in the Sandwich Islands) earthquake shocks were felt at several points in New Zealand, two occurring at Napier at $9^h\ 55^m$; Castle Point, $9^h\ 56^m$; Lyttleton and Nelson, $9^h\ 57^m$.

In eastern Australia there were unusual waves and tidal disturbance. At Sydney it was high water at 5^h on the 15th of August, and it was ebbing fast, when at 8^h it suddenly turned and ran with great force up the harbor, snapping the warps of one of the ferries, and creating much confusion. At one point the water rose two feet in five minutes. At Newcastle the ship Alexander was swept from her moorings; the water rose $2\frac{1}{2}$ feet in 15 minutes. Similar disturbances were experienced in Tasmania, Queens-

land and New Zealand. It is stated also that at the same time two slight shocks of earthquake were felt at Wellington, N. S. W., and at some other places; but whether on the same day precisely is not quite clear.

2. *Earthquake in California, Oct.* 21.—On the 21st of October, at 7h. 50m. in the morning, an earthquake occurred, doing considerable damage to houses, in San Francisco and Oakland. At the corner of Market and First streets, San Francisco, the ground opened several inches wide, for a distance of 40 or 50 feet. Another shock was felt in the city at 7 P. M.

The earthquake was also severe in the interior. Shocks were felt at Sacramento and Stockton; at Redwood City, where the court house is a wreck; at Marysville, slight; at Grass Valley, severe; at Sonora, light, but continuing nearly all day.

Other shocks are reported as having occurred at San Francisco on October 23d, 25th and 27th.

3. *List of Papers read at the session of the National Academy of Sciences in August*, 1868, *at Northampton.*

1. On the Tides and Tidal Currents of Hell Gate; Henry Mitchell, Assistant Coast Survey. Read by Prof. Peirce.

2. On the Rainfall of the United States; Chas. A. Schott, Assistant Coast Survey, from data collected by the Smithsonian Institution; paper presented by Prof. Henry.

3. Remarks on Mr. Airy's reduction of Kirchhoff's scale; Wolcott Gibbs.

4. On the origin of bitumens, together with experiments upon the formation of Asphaltum; J. F. Peckham.

5. On the Topography and topographical work west of the 103d meridian; J. D. Whitney.

6. On an invariable meter; A. C. Twining.

7. On the Tides and Tidal currents in an estuary or a canal between two tidal stations; Benjamin Peirce.

8. On deep sea dredgings in the Gulf Stream; L. F. Pourtales.

9. On subdivisions of Cretaceous and Marine Tertiary formations in California; W. M. Gabb.

10. On the constitution of Uric Acid and its derivatives; Wolcott Gibbs.

11. On new methods in Analytical Chemistry; Wolcott Gibbs.

12. On the fossil human skull of Calaveras county, California; J. D. Whitney.

13. On the distribution of the Forest Vegetation west of the Rocky Mts.; W. H. Brewer.

14. On the reported occurrence of human bones at Antelope station, Union Pacific Railroad; O. C. Marsh.

15. On the motions of a freely suspended pendulum; J. G. Barnard.

16. Tides and tidal currents in a harbor; Benjamin Peirce.

17. On alphabetic systems as tests of race; W. D. Whitney.

18. On the estimation of Carbonic acid; S. W. Johnson.

19. On Nitrification; S. W. Johnson.

20. On a new Borate from Mine Hill, Sussex Co., New Jersey; G. J. Brush.

21. Some points in the Surface Geology of the region west of the Rocky Mts.; J. D. Whitney.

22. On the transportation of the materials of the Carboniferous Conglomerate; J. S. Newberry.

23. On the deposits of Sulphate of Strontia on Strontian Island, Lake Erie; J. S. Newberry.

24. A new and general method of developing the real roots of an equation, essentially extracted from my algebra, page 512; Theodore Strong.

25. Remarks on the pendulum and gyroscope as exhibiting the rotation of the earth; J. G. Barnard.

26. On some interesting points in the structural geology of the Nile Valley; J. P. Lesley.

4. *On a Meteorite which exploded over Kansas, June* 6, 1868; by Prof. B. F. Mudge, (communicated for this Journal.)—At twenty minutes before noon, June 6th, a most brilliant meteorite was seen in our western sky. It exhibited a pink flame, shining with so vivid a light, that had it occurred in the night it would have obscured the full moon. It descended at an angle of 75° with the horizon, leaving a streak or sharply defined line of its track, which continued nearly a minute. When first seen by me, it had an elevation of 55°, though others saw it still higher. The diameter of the nucleus was about 15′ or nearly half the diameter of the moon. It descended with great velocity, (in less time than one second) and when 12° above the horizon, bearing 10° N. of W., exploded, sending two streams of fire toward the earth. The report of the explosion followed in about four minutes and a half, (owing to inadvertence the time was not exactly noted.) The detonation was double but following each other in quick succession, nearly simultaneous. Here it sounded as loud as a twelve-pounder a mile distant. Those west of us, who heard it, described it as more like sharp thunder. A farmer in the Republican valley, fifty miles from this place, supposed it was the steam boiler of a mill, and hurried to the spot to learn the damage done. On exploding, it produced a light blue cloud one and a half degrees long and two-thirds as wide, which floated in sight seventeen minutes without much diminution in size, when a passing cirro-cumulus cloud obscured it. The report was heard over an area 120 miles in diameter and the meteorite was seen much farther, viz., at Topeka, Marysville, Fort Harker, Fort Zarah and intermediate points. Its flight was apparently from south to north, though its motion was so rapid and nearly perpendicular, that it was difficult to determine.

The following are its elements, very nearly:

Height when first seen,	81 miles.
" " it exploded,	12·5 "
Length of the cloud caused by the explosion,	1·44 "
Breadth "	·96 "
Size of the neucleus or head,	1,890 feet.
Distance from this place when it exploded, about	58 miles.

It must have exploded over the country about midway between the Republican and Solomon rivers, which has few inhabitants. An aerolite must have fallen and I have spent some time in endeavoring to find it, but thus far without success.

State Agricultural College, Manhattan, Kansas.

5. *Corrigenda for Article* XIV *on Vision.*—In the article on Vision by S. Rowley in our September number (p. 153), notice of two omissions in the statement of the proposition was forwarded by the author, but reached us too late for insertion. The following are the corrections to be made:

In 8th line from bottom of page 155 (counting the lines of the note), between the words *behind* and *the*, insert the words, *the center of.*

In 4th line from top of page 156, between the words *plane* and *perpendicular*, insert the words, *passing through the middle of the interval between the eyes and the point of intersection of the optic axes.*

6. *British Association.*—The next meeting of the British Association will be held at Exeter, under the presidency of Prof. G. S. Stokes, Sec. R. S.

V. MISCELLANEOUS BIBLIOGRAPHY.

1. *Report of* J. Ross Brown *on the Mineral Resources of the States and Territories west of the Rocky Mountains.* Washington: Government Printing Office, 1868. 8vo, pp. 674.—We find in Mr. Brown's Report a large mass of important and interesting information which it is desirable should be accessible in a compact form for the use of all who wish a resumé of the existing state of development as well as of the general nature and extent of the vast mineral regions of the U. S. *west* of the Rocky Mountains.

As Mr. Brown is neither a mineralogist, geologist nor miner, we can fairly look in this document only for such information as an industrious collector of facts and an experienced traveler may amass upon almost any subject to which he brings zeal and intelligence. We can readily overlook numerous slips in matters technical and scientific, in view of the substantial contribution Mr. Brown has made to our literature upon this subject. The want of good maps (or maps of any kind), of plans and sections in Mr. Brown's Report, we regret the less, now that we know that these are being supplied in a manner which can hardly fail to be satisfactory by the labors of the party under the guidance of those experienced topographical and geological explorers, Messrs. King and Gardner, whose survey of the 40th parallel covers a zone of country reaching from the Sierra Nevada to the eastern slopes of the Rocky Mountains, within which fall the most important mining districts of the great interior basin of the continent. It may be two or three years yet before we get the full returns of this large and well organized party, but the volume on the mining regions of Nevada, Montana, Idaho, Utah, and Colorado, which embraces the labors of the mining engineer, Mr. James D. Hague, charged with this

special department, will, we understand, be in readiness for presentation, this coming session, to Congress.

About one half of Mr. Brown's Report is occupied with a detailed notice of the gold region of the State of California. The silver regions of Nevada occupy the next important place; after which, Arizona, Utah, Montana, Idaho, Washington Territory, Oregon and Alaska are discussed; followed by general observations on the "Pacific Slope;" general considerations on the precious metals; foreign States and Territories, and an Appendix, containing various official and leading documents relative to mining.

On the whole, we rise from an examination of Mr. Brown's Report with a feeling that he deserves praise for good sense and intelligence, which he has brought to his difficult task. This good sense he has shown in nothing more than in the use he has made of the labors of others who have preceded him in the same fields, labors generally, but not always, properly acknowledged. The chapters on the Sutro Tunnel, the general considerations on the precious metals, and that on Lower California and on Mexico, are those which possess perhaps the highest permanent value of any portion of the Report. Under the same covers with Mr. Brown's Report, we find a

2. *Report of* JAMES W. TAYLOR *on the "Mineral Resources of the United States east of the Rocky Mountains."* pp. 71.—A more utterly incompetent and wretched discussion of a great theme never disgraced a Governmental Blue-Book. Of its 71 pages, barely 28 are from the pen of its reputed author; and if the whole of this hotch-potch of "Canadian Mines," "Nova Scotia," Metallurgical Treatment, Taxation, Transportation, and Foreign Railways, had been omitted, the value of the document would not have suffered. The Appendix, which forms more than one half of the Report, reproduces an old Essay on Artesian Wells, by D. D. Owen, Dr. Hayden's late paper on the Western lignites, from this Journal, a fair essay on the mineral resources of Montana, by W. S. Keys, M. E., and a long letter from Mr. Gold-Commissioner Hamilton of Halifax on the gold and coal mines of Nova Scotia. What the United States may have to do with the Gold Regions of Canada, and the Gold and Coal regions of Nova Scotia, does not appear from the context, although it is evident that their annexation to Mr. Taylor's Report fills an important void.

We cannot refrain from expressing our surprise and amazement that our Government should intrust a commission so important as that of preparing a Report on the Mineral Resources of the States and Territories East of the Rocky Mountains to such unworthy hands. The country abounds with men who have devoted their lives to the study of our geological and mineralogical resources, and whose published works bear ample testimony to their ability to serve their country and advance science. In passing by these men and selecting the author of the document before us, the Government places the country in a false position before the whole world, and fails utterly in the only object it can have in the production of such official Reports.

3. *A treatise on the Concentration of all kinds of Ores, including the Chlorinatian Process for Gold bearing Sulphurets, Arseniurets and Gold and Silver ores generally;* by GUIDO KUSTEL. 259 pp. 8vo, with 120 diagrams on 7 plates. (San Francisco: office of Mining and Scientific Press. New York: Western & Co., office of American Journal of Mining.)—Mr. Kustel has already done great service for practical metallurgy by the publication of his book on the "*Nevada and California processes of Gold and Silver extraction*," and the present work is one of much importance to both miners and metallurgists. It is, we believe, the only work of the kind in the English language, and besides original material, gives us the latest results attained abroad, and especially much of the valuable matter contained in the recent elaborate work of von Rittinger. It is very systematically arranged—an introductory chapter treats of the general principles of ore dressing, the separation from the gangue in and outside the mine, ragging, spalling, cobbing, etc.; the different varieties of cleansing apparatus, and the various kinds of rotary sizers. The second division of the subject entitled "*reduction*," treats of the different breakers, crushers, and pans, with a proper consideration of the theoretical points involved in the construction of these machines. The third and fourth sections of the book are devoted to "*concentration*" and "*special concentration*," under which the principles involved in concentration are discussed, with descriptions of the various jigs, setz-machine, setz-rad, buddles, sleeping and percussion tables; in short, the principal machines and methods employed in ore concentration. The fifth division is devoted to the *chlorination process*, and is the most complete presentation of the subject that has yet been published. As before stated, the work contains much that till now was inaccessible to the English reader, and we believe that a careful study of the principles contained in it will be of great service to all interested in this department of practical science. G. J. B.

4. *The American Annual Cyclopedia and Register of Important events of the year* 1868. Vol. vii. 8vo, pp. 799. New York, 1868. (D. Appleton & Co.)—Like its predecessors, this portly annual volume is devoted to bringing down to date all the more important events of the year 1867, political, civil, military, and social affairs, public documents, biography, statistics, commerce, finance, literature, science, agriculture and mechanical industry. The ample space of these volumes admits a degree of fullness in the discussion of important subjects which gives to the prominent articles the character of separate treatises. The articles on scientific subjects are of course not very numerous. Those of most prominence are chemistry, electricity, geographical explorations and discoveries, French Exposition, magnesium, medicine, metals, meteors, sulphur produce of Italy, and test of iron by magnetism. The volume closes by a useful analytical table of contents, in addition to the usual index of subjects.

5. *The Wine Maker's Manual;* by CHARLES REEMELIN, author of the "Vine Dresser's Manual." 123 pp. 12mo. Cincinnati, 1868.

(Robert Clarke & Co.)—This little manual is especially intended as a guide to small farmers and others who wish to make wine in small quantities, whether from grapes or other fruits. The author is evidently a person well acquainted with the practice of his art, and treats his subject in a plain and conscientious manner. The English of the book would have been improved by a little friendly review, and the same may be said of its chemistry. But the notions sought to be conveyed are good and will meet the demand from the class to which it is especially addressed. The mechanical production of the book is extremely tasteful. The author, like all other writers of books on the grape and wines east of the Mississippi, seems to forget that California is one of the States of the American Union. The only paragraph in the book in which California is mentioned, speaks of Coutro Krota County, meaning Contra Costa County, a blunder which any gazetteer would have corrected. But the climate, vine and soil of California are so entirely unlike anything in the Eastern United States, and especially in their peculiar adaptation to the production of the grape and its wine that we can hardly include them in a book intended for those who cultivate the vine of the eastern United States.

6. *Chambers's Encyclopedia: a Dictionary of Universal Knowledge for the people.* (Illustrated.) Vols. viii–x. Philadelphia: Lippincott & Co.; Edinburgh: W. & R. Chambers. Large 8vo. 1867–1868.—The tenth volume completes this excellent work, the progress of which we have noticed from time to time in former volumes of this Journal. Its character for fullness, accuracy and general interest is well maintained to the end. Over 27,000 articles, comprising nearly every department of human knowledge, are in its alphabetical list. Its natural history articles, and those on the various physical and chemical sciences, are numerous and well illustrated by good wood-cuts in the text. Particular attention seems to have been given to those subjects which have an interest for Americans. Its biographical articles are numerous and embrace the living as well as the dead.

In an appendix which fills two-thirds of the final volume, many articles are added to keep pace with the rapid progress of science in its various departments. The article on chemistry brings forward in a manner on the whole pretty satisfactorily, the modern chemical philosophy, recognizing fully its importance, and rapid strides toward supremacy. Sir Benjamin Brodie's Calculus of Chemistry is not forgotten. It is a singular proof of the slowness with which the Anglo-Saxon mind adopts new ideas, that in 1868 it is still deemed essential in a leading exponent of the existing state of knowledge, to give in connection with the article chemistry an elaborate exposition of the metrical system of weights and measures, now for a generation in almost exclusive use among all scientists out of England. As is unavoidable, we find no mention of many subjects which might very naturally be looked for, but on the whole, he will be a captious reader who does not find in Cham-

bers and its supplement more than he can inwardly digest. In the appendix many of the scientific and mechanical novelties of the last few years are well treated of, such for example, as the Gas-Engine; Electro-magnetic Engine of Wilde; Blowing machines including Root's of the U. S.; Armorer Plates; Artificial Limbs; Breech-loading arms (deficient in no mention of important American forms); Cattle Plague; Chemistry; Coal Supply; Cohesion Figures; Compressed AirEngine; Dipsomania; Floating Docks; Meteors; National Education (an elaborate and important paper); Nitro-glycerine; Refrigerating Machines; [Here again the American researches of Twining reproduced in the English Ettier refrig- and the air rarefactoire of Gerrie described in a series of articles in this Journal more than 20 years ago, are not mentioned]; Sanitary Science; Thermo Dynamics, are examples of subjects discussed. We do not know if it is the purpose of the publishers to maintain the status of the excellent Encyclopedia by the addition of an annual volume as has been done by the Appletons for the American Encyclopedia,

7. *Mitchell's Manual of Practical Assaying*, 3d edition. Edited by WILLIAM CROOKES, F.R.S. London. 8vo, pp. 697, and lvii of tables. London, 1868 (Longmans, Green & Co).—Mitchell's Assaying is a book by far too generally known to require any extended notice; but the appearance of the third edition of Mr. Crookes's reminds us that it is fourteen years since the second edition (1854) was published, and in that time many important discoveries and improvements have been made in the various methods of chemical and metallurgical analysis, which are now well brought up by Mr. Crookes. The volumetric and colorimetric methods of assay have especially given precision and rapidity of execution to many cases of analysis before difficult and uncertain. These, as well as the blowpipe assay by the latest methods of Prof. Kerl, are found fully set forth in this edition, which thus becomes the best manual for practice in our language. The use of the old English weights and measures is still adhered to, as indeed is necessitated by the law requiring assay reports to be made in ounces, pennyweights and grains, all the gold and silver tables being rendered in the same terms. In the volumetric assays, however, the rational methods of the metrical system are employed.

8. *The Workshop: A monthly Journal devoted to the progress of the useful arts.* E. Steiger (17 William st., N. Y).—We have before announced Mr. Steiger's reproduction of this excellent German Art Journal, edited by Professors Baumer and Schnor. With the completion of the first six months the Workshop appears in an enlarged form, evincing a determination to keep well up with the growing taste for genuine art culture. It is a work which commends itself to all whose tastes or vocations lead them in this direction. The articles are original, critical and suggestive; the illustrations varied, numerous and beautifully executed, and are accompanied by working drawings which every artist will appreciate as bringing to a practical test the examples given. To the German more than to any other people do we owe the recent rapid

progress of correct ideas and sound principles in art; but until now the English reading artists have had very limited means of information in this direction. This want the Workshop does much to supply, and we cheerfully commend it to general attention.

9. *Thesaurus Siluricus: The Flora and Fauna of the Silurian Period, with Addenda* (from recent acquisitions); by John J. Bigsby, M.D., F.G.S., formerly British Secretary Canadian Boundary Commission, etc., etc. 214 pp. 4to. London, 1868. (J. Van Voorst.)—This work is a condensed and comprehensive review of the facts in Silurian paleontology. It is the result of a vast amount of labor on the part of one who has examined for himself the Silurian rocks of a portion of North America, and studied carefully the investigations of others all over the world. The subjects are discussed with ability, evidently after careful study, and with the aid of some of the best paleontologists of England. The principal topics considered are zoological considerations, geographical distribution, locality, time of first appearance of species, duration and extinction, migration and recurrence. The tables include, the author states, 997 species, which is more than four times as many as were comprised in Bronn's admirable tables of 1856. To no geologists has the work greater interest than to those of North America.

10. *Exposé des Formations Quaternaires de la Suède;* par A. Erdmann. 118 pp. 8vo, illustrated by 26 wood cuts, with an atlas in quarto of 14 plates.—Prof. Erdmann has here presented an abridgment "pour l'étranger" of his larger work on the Quaternary or Post-tertiary formations of Sweden. The facts are closely related to those of North America, and throw much light, therefore, on American geology. Prof. Erdmann divides the era into the Glacial and Post-glacial. The Glacial includes two epochs, that of glaciers, and that of submergence and submarine deposits. The Post-glacial is also divided into two epochs, 1, that of submarine deposits; 2, that of the more recent terrestrial and submarine beds. The several subdivisions are treated of with much interesting detail. The quarto atlas is one of great beauty, containing, besides sections, several maps illustrating the distribution of the deposits, the direction of scratches, etc.

11. *Prof. Safford's report on the Geology of Tennessee.*—Prof. Safford writes that he is engaged in the preparation of his report, and that he hopes to have it out in an abridged form in January.

12. *The American Entomologist.* Edited by Benj. D. Walsh of Rock Island, Illinois, and C. V. Riley of St. Louis, Missouri. Vol. I, No. 1, September, 1868. 20 pp. large 8vo.—This is the first number of a new Entomological Journal published at St. Louis. Its aim, as stated in the prospectus, is to promote the interests of Entomology, and especially to disseminate practical information to farmers, gardeners, fruit growers and others. Its editors are fully capable of sustaining well the Journal, and this first number holds out excellent promise for the future. The price per year is but one dollar.

13. *How Crops Grow. A Treatise on the Chemical Composition, Structure and Life of the Plant, for all Students of Agriculture, with numerous illustrations and tables of analyses;* by SAMUEL W. JOHNSON, M.A., Professor of Analytical and Agricultural Chemistry in the Sheffield Scientific School of Yale College, &c. 394 pp. 12mo. New York, 1868. (Orange Judd & Co.)—Prof. Johnson is so completely master of his subject that anything coming from his pen carries with it the prestige of authority. This volume well sustains the author's established reputation. It is the result of much study and experience as an instructor, and is specially adapted to a large class of readers whether interested in pure or applied science. It is to be followed by two and perhaps three other volumes in the same line; the second of the series, to be entitled "How Crops Feed," is nearly ready for the printer; the third volume will be upon Cultivation, or the Improvement of the Soil and the Crop by Tillage and Manures. The closing volume of this important series will be devoted to Stock Feeding and Dairy Produce considered from the point of view of Chemical and Physiological Science. It is easy to see how valuable such a series must be to all who are interested in agriculture in its various departments, and to scientific readers they will furnish a reliable epitome of the sciences discussed.

14. *A Treatise on Optics; or Light and Sight theoretically and practically treated; with the application to fine art and industrial pursuits;* by E. NUGENT, C.E. With one hundred and three illustrations. 8vo, pp. xii, 285. New York, 1868. (D. Van Nostrand.)—Mr. Nugent has succeeded in condensing into this little book a large amount of valuable information. While he has not burdened its pages with abstruse formulas, he yet has been able very clearly to demonstrate the principles of refraction, reflexion, dispersion, polarization, etc. The opening chapter gives a succinct history of optical science; the next fourteen chapters are occupied with a discussion of theoretical optics; in the sixteenth a description is given of the more important optical instruments, as the camera—giving figures of the best lens combinations of Dallmeyer and others—the microscope with its appliances, the telescope, stereoscope, sextant, theodolite, engineer's transit, etc.; and in the seventeenth many important practical applications of optical principles are discussed; among them photo-zincography, photolithography, the influence of colored light on the germination and growth of seeds, and light-house illumination. The matter of the book is well digested, the illustrations are good, and the style in which it is issued is unexceptionable.

15. Part 2 of the *Butterflies of N. America*, with beautiful colored plates, by WM. H. EDWARDS, has just been published in Philadelphia.

NOTE.—*Illustrations of this volume.*—For the illustrations of the article on the Eozoon (p. 245) we are indebted to the liberality of Sir Wm. Logan; and for those of the article on the secular variations of the Earth's Orbit by J. L. Stockwell (p. 87), on the Amiens Gravel by Tylor (p. 302), and on Siredons by Prof. O. C. Marsh (p. 364), to the several authors.—EDS.

INDEX TO VOLUME XLVI.

A

Academy, National, meeting, 1868, 428.
Nat. Sci. Philad., Proceed., 152.
St. Louis, Transactions, 288.
Air-rings, visible, *Ball*, 260.
American Assoc., Proceedings, 1866, 151; 1867, 287; 1868, 148, 275.
Naturalist, noticed, 286.
Ammonia, nitrite of, *Loew*, 29.
Andes, observations on, *Orton*, 203.
Antimony, sulphates of oxyd of, *Dexter*, 78.
Appleton's Annual Cyclopedia, 432.
Arnott, G. A. W., obituary, 273.
Asteroids, (91) Ægina, 147.
(93) Minerva, 147.
(94) Aurora, 147.
(95) Arethusa, 147.
(96) , 147.
(97) Clotho, 147.
(98) , 274.
(99) , 274.
(100) Hecate, 393.
(101) Helena, 274, 393.
(102) Miriam, 275, 392, 393.
(103) , 392, 393.
(104) , 392, 393.
(105) , 392, 393.
(106) , 393.
Astronomy, Lockyer's, noticed, 275.
Watson's, noticed, *Newton*, 145.
Auroras, Sept., 1868, in Me., *Gilman*, 390.

B

Ball, R., air-rings, 260.
Barker, G. F., chemical abstracts, 129.
notices in physiological chemistry, 233, 379.
Bayma's Mechanics, *Norton*, 167.
Bernard on sugar in the liver, 379.
Berthelot on saturating organic bodies with hydrogen, 125, 395.
on carbonylic sulphid, 129.
Bickmore, A. S., journey through China, 1.
Bigsby's Thesaurus Siluricus, 435.
Bone caves of Brazil, *Reinhardt*, 264.
Bost. Nat. Hist. Soc. Memoirs, 152.
Proceedings, 152, 288.
BOTANY—
Algæ from hot springs, *Wood*, 31.
Nebraska Cretaceous plants, 91.
N. Am. cretaceous and tertiary flora, 401.
Nomenclature, laws of, at Botan. Congress, 63.
remarks on, *Gray*, 74.
BOTANICAL WORKS noticed:—
Brown, R., works, v. II, 271.
DeCandolle, A., Prodromus, 408.
DeCandolle, C., Théorie de la feuille, 272.
Hoopes, Book of Evergreens, 270.
Linnean Soc. Journal, 271.
Martius, Flora Brasiliensis, 408.
Schmidt, Reisen in Amur-lande, 408.
Braithwaite's Retrospect, noticed, 286.
Brewer, W. H., notice of Darwin's Variation under domestication, 140.
British Assoc., meeting in 1868, 148.
in 1869, 430.
Brown, R., botan. works, noticed, 271.
Browne, J. R., on Mineral resources of U. S., noticed, 430.
Brush, G. J., sussexite, 140, 240.
Kustel on Ores, 287, 432.

C

California earthquake, Oct., 1868, 428.
human remains in, *Winslow*, 407.
Calomel, vapor-density of, 398.
Cambridge Physics, noticed, 285.
Carbon, bisulphid of, in sunlight, 363.
Carpenter, W. B., Eozoon Canadense, 245.
Caucasus, notes on, *v. Koschkull*, 214, 335.
Cerium, equivalent of, *Wolf*, 53.
Chambers's Encyclopedia, 433.
Chase, P. E., rain-fall affected by the moon, 281.
connotations of magnetism, 398.
Chemistry, Eliot and Storer's, noticed, 130.
physiological, notices, *Barker*, 233, 379.
Cheyney, J. S., mode of showing vibrations, 243.
China, journey through, *Bickmore*, 1.
Chladni plates, mode of showing the vibrations in, *Cheyney*, 243.
Chromium, mode of obtaining, 131.
Coal, see GEOLOGY.
Coan, T., eruption in Hawaii, 106.
Combustion under pressure, 394.
Cope, E. D., new Enaliosaur, 263.
on genus Lælaps, 415.
Crookes's ed. of Mitchell on assaying, 434.
Crowther, R., petroleum in Mex., 147.

D

Dana, J. D., eruption in Hawaii, 105, 123.
Mineralogy, 5th ed., noticed, 132.
Darwin's Variation under domestication, *Brewer*, 140.
Dawson, J. W., Eozoon Canadense, 245.
new Zonites, 265.
Acadian geology, noticed, 267.

Debray on compounds of molybdic and phosphoric acids, 397.
DeCandolle, A., laws of nomenclature at Botan. Congress, 63.
Prodromus, noticed, 408.
DeCandolle, C., Théorie de la feuille, 272.
Dexter, W. P., chemical apparatus, 51.
sulphates of antimony, 78.

E

Earth's orbit, secular variation in elements of, *Stockwell*, 87, 436.
Earthquake in S. A., Aug., 1868, 422.
in Cal., Oct., 1868, 428.
in Australia, Aug., 427.
Edwards's Butterflies of N. A., 150, 436.
Eggs, coloring matter of, *Städeler*, 235.
Electricity, Faraday's discoveries in, *Tyndall*, 34, 180.
Eliot and Storer's Chemistry, noticed, 130.
Entomological Soc., Philad., transactions, 150, 151, 288.
Entomologist, American, noticed, 435.
Eozoon Canadense, see GEOLOGY.
Erdmann's Exposé des Formations Quaternaires de la Suède, noticed, 435.
Essex Inst. Proceed., 288.

F

Faraday as a discoverer, *Tyndall*, 34, 180.
Fishes destroyed in Bay of Fundy, 269.
Fossil animals, see GEOLOGY and ZOOLOGY.
plants, see BOTANY.
Frankland on combustion under pressure, 394.

G

Gas lamp and regulator, *Dexter*, 51.
Geinitz on a German meteorite, 284.
GEOLOGY—
Amiens gravel, *Tylor*, 302, 436.
Bone caves, Brazil, *Reinhardt*, 264.
Denudation, subaërial, *Whitaker*, 268.
Enaliosaur, new, *Cope*, 263.
Entomostraca, Paleozoic, 265.
Eozoon Canadense, *Dawson* and *Carpenter*, 245, 436.
Ontario, geol. of, *Hunt*, 355.
Vt., geol. notes on, *Hunt*, 222.
Zonites, new, *Dawson*, 265.
GEOLOGICAL WORKS noticed:—
Bigsby, Thesaurus Siluricus, 435.
Dawson, Acadian Geol., new ed., 267.
Delesse et *Lapparent*, Revue de Géol., 270.
Erdmann, Exposé des Formations Quaternaires de la Suède, 435.
Kjerulf and *Dahll*, chart of S. Norway, 269.
Maps in preparation, 140.
Safford, Tenn. Geol. rep't., 435.
Gibbs, W., chem. abstracts, 124, 258, 394.
uric acid, 289.
Gilman, W. S., auroras in Me., 390.
Gold mining, Phillips on, noticed, 134.
ores, Kustel on, *Brush*, 287, 432.
Gray, A., remarks on laws of botanical nomenclature, 74.
botanical notices, 270, 408.
necrology, 273.
Gulf Stream fauna, *Pourtales*, 409, 413.

H

Hæmatoidin, Holm on, 233.
Hall, J., geol. map of U. S., by, 140.
paleontological report, noticed, 262.
Hansen on moon's constitution, *Newcomb*, 376.
Hawaii, eruption in, 105.
Heintz on formula of urea, 237.
Hennessey, J. H. N., rain-fall affected by the moon, 283.
Hetch-Hetchy valley, *Hoffmann*, 266.
Hill, T., on the occultator, 299.
Hillebrand, W., eruption in Hawaii, 115.
Hinrich's Atom mechanics, *Fleck*, 258.
Hitchcock, C. H., N. Am. geol. atlas by, 140.
Hoffmann, C. F., Hetch-Hetchy valley, 266.
Hofmann on persulphid of hydrogen, 396.
Holm on hæmatoidin, 233.
supra-renal glands, 236.
Hoopes on Evergreens, noticed, 270.
Human remains, see Man.
Hunt, T. S., geology of Vt., 222.
geology of Ontario, 355.
Hyatt on Polyzoa, noticed, 150.
Hydrogen, saturation with, *Berthelot*, 125, 395.
persulphid of, *Hofmann*, 396.

J

Johnson, S. W., Eliot and Storer's Chemistry, 130.
How Crops grow, by, noticed, 436.

K

Kölliker on polymorphism of anthozoa, and structure of tubiporæ, 273.
Koschkull, F. v., on the Caucasus, 214, 335.
Krantz's Catalogue, noticed, 287.
Kustel on Ores, noticed, 287, 432.

L

Lang's Astron. Tables, noticed, 275.
Lartet and Christy's Reliquiæ Aquitanicæ, 287.
Lehmann on sugar in the blood, 380.
Leipzig Astron. Gesellsch., publications, 288.
Lesquereux, L., Nebraska cretaceous plants, 91.
Liver, sugar in the, 379.
Lockyer's Astronomy, 275.
Loew, O., nitrite of ammonia, 29.
bisulphid of carbon, in sunlight, 363.
Logan, W. E., Eozoon Canadense, 245, 436.
Loomis's Meteorology, noticed, 149.
Loughlin, J. E., molybdenum and chromium, 181.
Lyman, C. S., Cambridge Physics, noticed, 285.
Lyman, F. S., eruption in Hawaii, 109.

M

Magnetism, connotations of, *Chase*, 398.
Magneto-electricity, see Electricity.
Man, remains of, in Cal. drift, 407.
Marsh, O. C., metamorphosis of Siredon, 364, 436.
new species of fossil horse, 374.
geol. map of U. S. by, noticed, 140.
Matteucci, C., obituary, 285.
Meek, F. B., Ethmophyllum and Archeocyathus, 144.
shell structure of spirifera, 262, 408.
Meek, F. B., and *A. H. Worthen*, on a fossil scorpion, etc., from Ill., 19.
Meteorite from Ga., *Shepard*, 257.
Germany, *Geinitz*, 284.
Kansas, *Mudge*, 429.
Meteorology, Loomis's, noticed, 149.
Mineral resources of U. S., Browne on, noticed, 430; Taylor on, noticed, 431.
Mineralogy, Dana's, 5th ed., 132.
MINERALS—
Aquacreptite, *Shepard*, 256.
Corundophilite, *Shepard*, 256; Cryolite, minerals in, *Rand*, 400.
Enargite from Cal., *Root*, 201.
Ivigtite found in Cryolite, *Rand*, 400.
Sussexite from N. J., *Brush*, 140, 240.
Tephroite from N. J., *Mixter*, 231.
Willemite from N. J., *Mixter*, 230.
Mining, Phillips on, *Silliman*, 134.
Kustel on, *Brush*, 432.
Mitchell on Assaying, new ed., 434.
Mixter, W. G., willemite and tephroite, 230.
Molecular mechanics, Bayma on, 167.
Molecular physics, principles of, *Norton*, 167.
Molybdenum, mode of obtaining, 131.
Molybdic and phosphoric acids combined, 397.
Moon's constitution, Hansen on, *Newcomb*, 376.
Mudge, B. F., Kansas meteorite, 429

N

Naphthalin, Vohl's test for, 130.
N. Y. Regents of Univ., 20th report, 262.
Newberry, J. S., extinct N. A. floras, 401.
Newcomb, S., Hansen on the moon's constitution, 376.
Newton, H. A., Watson's Astronomy, 145.
Nobert's test-plate, *Woodward*, 352.
review of Stodder on, *Sullivant*, 347.
Norton, W. A., principles of molecular physics, 167.
Nugent's Optics, noticed, 436.

O

OBITUARY—
G. A. W. Arnott, 273.
M. Faraday, *Tyndall*, 34, 180.
C. Matteucci, 285.
C. G. Page, 149; J. Plücker, 149.
A. Rosing, 148.
N. B. Ward, 273.
Occultator, description of, *Hill*, 299.
Organic substances, artificial formation of, *Williams*, 327.
Orton, J., observations on the Andes and Amazons, 203.

P

Packard on study of insects, noticed, 274.
Page, C. G., obituary, 149.
Paleontology, see GEOLOGY and ZOOLOGY.
Petroleum in Mexico, *Crowther*, 147.
Phillips on Mining and Metallurgy of Gold and Silver, noticed, *Silliman*, 134.
Phosphoric and molybdic acids, 397.
Physiological chemistry, notices in, *Barker*, 233, 379
Planets discovered, see Asteroids.
Plücker, J., obituary, 149.
Pourtales, L. F., Gulf Stream fauna, 409, 413.

R

Rain fall affected by the moon, *Chase*, 281; *Hennessey*, 283.
Rand, T. D., ivigtite, 400.
Ranking's Med. Abstract, noticed, 286.
Reemelin on Wine making, 432.
Reinhardt on Brazil bone-caves, 264.
Rolfe and Gillet's Cambridge Physics, 285.
Root, E. W., enargite from Cal., 201.
Rosing, A., obituary, 148.
Rowley, S., theory of vision, 153, 430.

S

Safford's Tenn. Geol. report, noticed, 435.
Sandwich, Is., eruption in, 105.
earthquake wave at, 426.
Scorpion, fossil, *Meek* and *Worthen*, 19.
Scudder, S. H., mole crickets, 417.
two Carboniferous insects, 419.
Sheffield Laboratory Contrib., 230, 240.
Shepard, C. U., on aquacreptite, 256.
corundophilite, Chester, 256.
meteorite from Ga., 257.
Silicon, iodid of, *Friedel*, 398.
oxychlorid of, 125.
Silliman, B., Phillips on Gold Mining, 134.
Amer. Assoc. meeting, 275.
transformation of Siredon, 421.
Silver mining, Phillips on, noticed, 134.
Siredon, metamorphosis of, *Marsh*, 364, 436; *Silliman*, 421.
Städeler on yolk of eggs, 235.
Stockwell, J. L., variation in elements of earth's orbit, 87, 436.
Stodder on Nobert's test-plate, review of, *Sullivant*, 347.
Strecker on sulphacids, 124.
uric acid, 124.
Sugar in the liver, 379.
Sullivant, W. S., notes on Stodder on Nobert's test-plate, 347.
Sulphacids, formation of, *Strecker*, 124.
Sulphid, carbonylic, *Berthelot*, 129.

T

Taylor on Mineral resources of U. S., 431.
Teyler Museum, Archives, noticed, 286.
Tylor, A., on the Amiens gravel, 302, 436.
Tyndall, J., sketch of Faraday, 34, 180.

U

U. S. Astron. and Meteorological Observations for 1865, 146.
Urea, formula of, *Heintz*, 237.
Uric Acid, *Gibbs*, 289; *Strecker*, 124.

V

Vermont, notes on geol., *Hunt*, 222.
Verrill, A. E., Alcyonaria in Y. C. Museum, 143.
  notice of Hyatt on Polyzoa, 150.
Vision, theory of, *Rowley*, 153, 430.
Vohl's test for naphthalin, 130.
Volcanic eruption, Hawaii, 105.

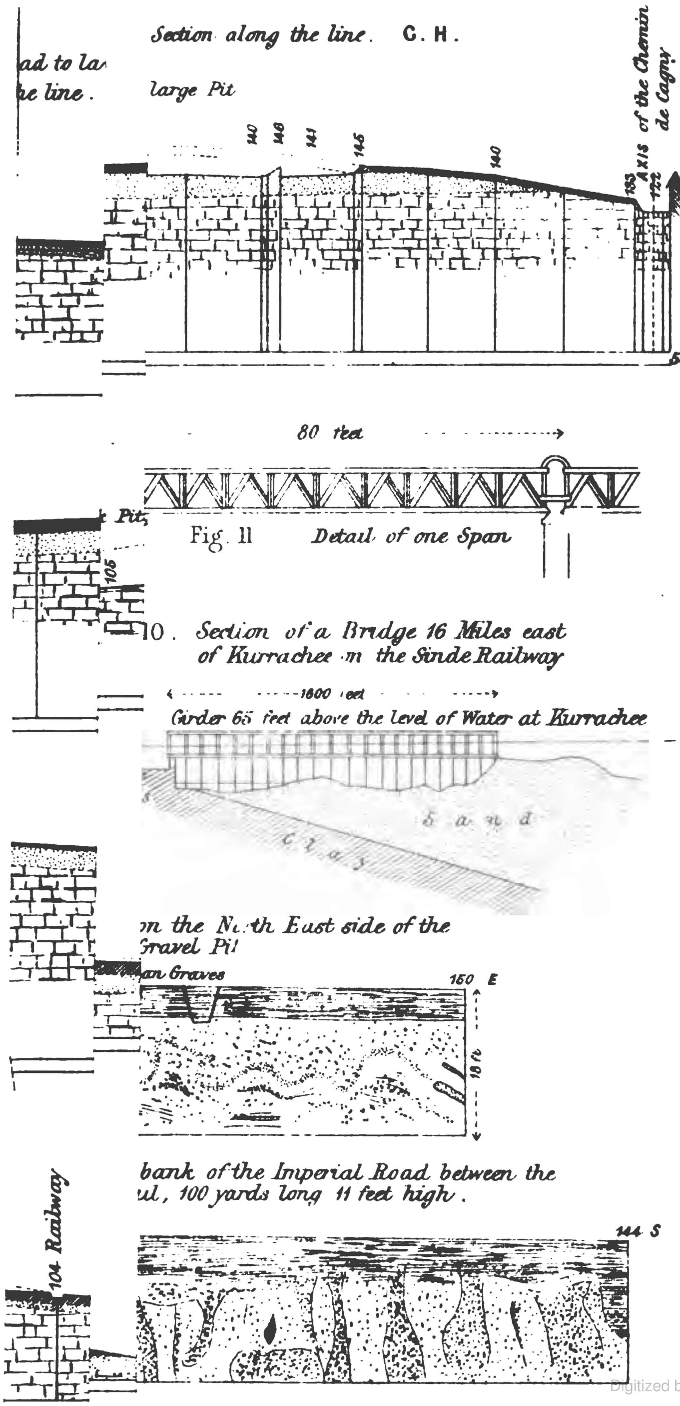
W

Ward, N. B., obituary, 273.
Watson, J. C., planets discovered, 274, 392.
  Theoretical Astronomy, noticed, 145
Whitaker, W., subaërial denudation, 268.
Whitney, H. M., eruption in Hawaii, 112.
Williams, C. G., artificial formation of organic substances, 327.
Winchell, A., geol. chart. by, 140.
Wine making, Reemelin on, noticed, 432.
Winslow, C. F., human remains in Cal., 407.
Wolf, C., equivalent of cerium, 58.
Wood, H. C., algæ from Cal. hot spring, 81.
Woodward, J. J., Nobert's test-plate, 352.
Workshop, The, noticed, 151, 434.
Worthen, A. H., see *F. B. Meek.*

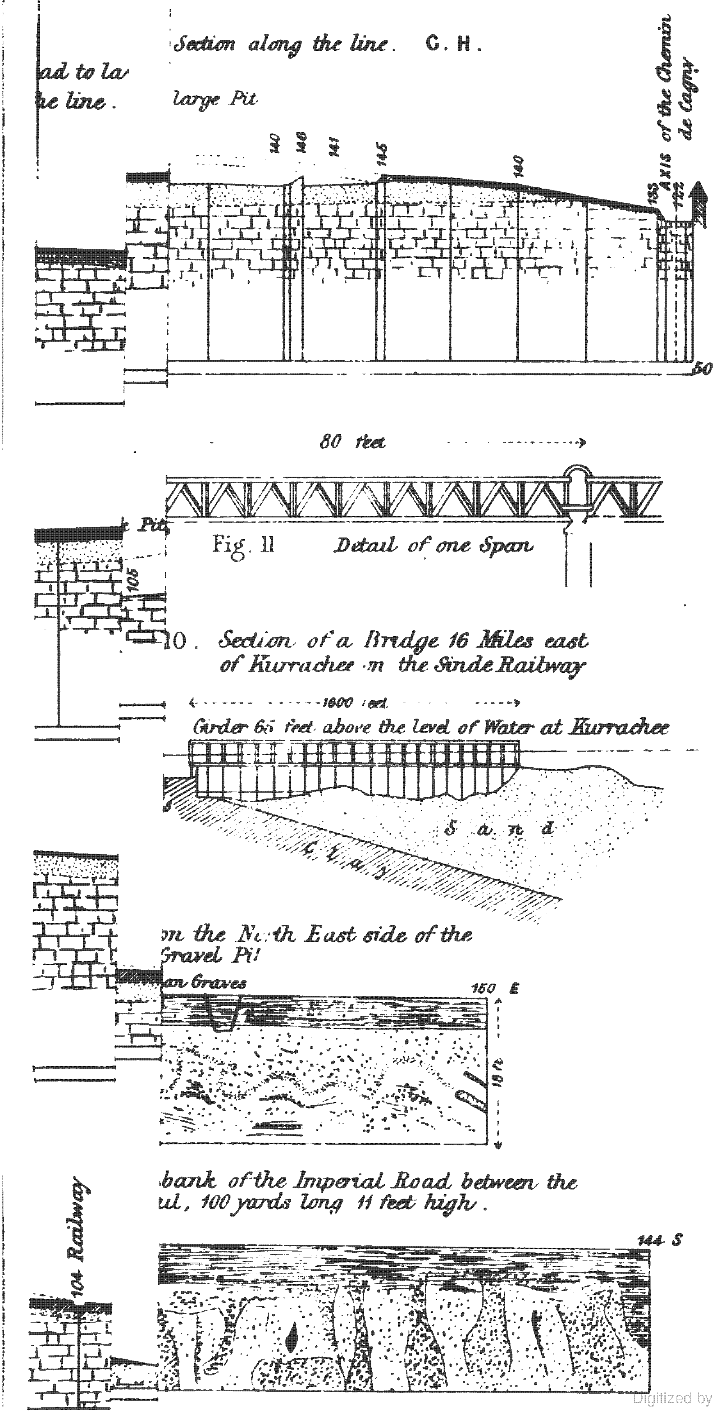
Z

Zoology—
  Alcyonaria in Y. C. Museum, *Verrill*, 143.
  Anthozoa, polymorphism of, 273.
  Carboniferous insects, *Scudder*, 419.
  Ethmophyllum and Archeocyathus, *Meek*, 144.
  Gulf Stream fauna, *Pourtales*, 409, 413.
  Horse, fossil, *Marsh*, 374.
  Lælaps, *Cope*, 415.
  Mole crickets, *Scudder*, 417.
  Scorpion, etc., from Ill., *Meek* and *Worthen*, 19.
  Siredon, metamorphosis into Amblystoma, *Marsh*, 364, 436; *Silliman*, 421.
  Tubiporæ, structure of, 273
Zool. Works, noticed:—
  Edwards, N. A. Butterflies, 150, 436.
  Hyatt, Polyzoa, 150.
  Packard, Study of insects, 274.


rg de
vais
Faubourg de
la Hotoie
St Maurice



F Dangerfield lith. 22, Bedford St Covent Garden.



F Dangerfield lith. 22, Bedford St. Covent Garden.